Economic Analyses of Cars in the City

Ostermeijer, Francis Jonathan

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This thesis provides a collection of four empirical analyses of the economic effects of cars in the city. Chapter 1 introduces the topics and the methodological approach. Chapter 2 evaluates the impact of a large citywide increase in hourly on-street parking prices in Amsterdam. Findings indicate a large reduction in parking demand and a moderate reduction in traffic demand. Chapter 3 examines to what extent residential parking prices affect car ownership decisions. It concludes that parking costs explain about one-third of the difference in car ownership rates between the urban centres and the periphery in the Netherlands. Chapter 4 finds that smartphone use explains about 10% of road accidents in the Netherlands and that these accidents mainly occur on local urban roads. Chapter 5 studies the overall long-term effect of cars on urban density. It finds that a large share of urban decentralisation over the twentieth century can be explained by car ownership rates. Chapter 6 concludes with an outline of the main results and a discussion of the key implications for urban transport policy and automated vehicles.

Francis Ostermeijer (1993) was born and raised in Indonesia, Portugal, and Azerbaijan. He completed his BSc in Economics at Tilburg University in 2014 and pursued two master studies at Erasmus University Rotterdam and the Vrije Universiteit Amsterdam, completing both in 2016. He has worked at the United Nations Industrial Development Organisation, lectured about Transport Economics at the University of Indonesia, and collaborated with policy makers at the municipality of Amsterdam to evaluate the impact of a major transport policy.

Doctoral thesis

ECONOMIC ANALYSES OF CARS IN THE CITY
IMPLICATIONS FOR POLICY AND AUTOMATED VEHICLES

Francis Ostermeijer

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VRIJE UNIVERSITEIT

ECONOMIC ANALYSES OF CARS IN THE CITY

IMPLICATIONS FOR POLICY AND AUTOMATED VEHICLES

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Preface

Since our earliest records, humans have been interested in seeing into the future. One famous account documents how the King of Lydia (an ancient Greek region in modern-day Turkey) solicited the council of the oracle of Delphi. King Croesus wanted to know what the outcome would be, should he seek war with the Persian empire. The oracle answered that if the King attacked the Persians, he would destroy a mighty empire. Interpreting this prediction in his favour, Croesus went into battle but suffered a crippling loss, ultimately fulfilling the prophecy and destroying a mighty empire – his own.

Several years later, the Athenians too went to the oracle to ask what they should do about the growing Persian threat. The oracle proclaimed an equally poetic and ambiguous prophecy, but the Athenians did what we should always do when faced with a serious forecast: they discussed and debated. They asked the important questions: what does this really mean, how seriously should we take it, and what should we do about it? The Athenians eventually decided to seek a naval battle and defeated the vastly superior Persian army at sea.

These days we laugh at prophets that proclaim to know the future, however we put a considerable amount of faith in mathematical models that aim to do just that. In many fields, including economics, better models and larger datasets have progressed our knowledge substantially. This collection of essays pursues this approach, with the intention to inform the current debate on urban and transport policy, while providing well thought-out estimates for the potential impact of automated vehicles (AVs). By learning from the past, I believe that we can design and imagine better policies for the future. However, we must not fall into the same trap as King Croesus and rather learn from the Athenian experience.

The results in this dissertation provide order of magnitude estimates for the impacts of AVs on parking, traffic, accidents, and urban density, but they are far from definitive. We, too, need to ask the important questions. Do we want AVs to drive around empty? How large are the safety gains and under which conditions do we expect them to arise? Should cities spread out further? Rather than prescriptive answers, I hope that these predictions can serve as a starting point for a serious discussion about these topics, along with how we want our cities to look like and the types of policies we could use in order to get there.

1 Introduction

The internal combustion engine and mass production of the automobile transformed cities in the twentieth century. In 2012, private motorised vehicles accounted for half of all passenger trips in large global cities and transported people ten times more kilometres than by public transport. On average, every household owned one car.[1] While this masks considerable heterogeneity between cities, countries, and regions, it clearly points towards cars as the dominant transport mode of the era. So how did cars become so widespread and *what are the implications for our cities*?

Cars solved many problems facing cities at the time, including pollution (horse manure), high commuting costs, and unaffordable housing. Compared to earlier technologies, cars offered more flexibility in terms of routes and schedules, lower marginal costs per kilometre, higher speeds, and more privacy. The adoption of the internal combustion engine was also aided by the availability of cheap fuel, mass production techniques, government support, and the perception towards transportation at the time (Sovacool, 2009). However, while the rapid adoption of cars in the mid-to-late twentieth century solved many urban issues, it brought about a new set of societal challenges, which we are still dealing with today.

Steadily increasing levels of car ownership have resulted in gridlock, unsafe levels of air pollution, environmental degradation, social exclusion, injury, and loss of life. Where cars promised breathtaking speeds, average traffic speed in many large cities is less than 30 km/hr, causing €270 billion in annual travel time losses in the European

[1] These statistics are based on the UITP (2015) dataset of 63 large global cities in 2012, predominantly in developed countries.

Union (EU) alone.[2] While our streets are clean from horse manure, cars instead pollute our air, which results in health problems and lower life expectancy causing an estimated annual loss of €35 billion in the EU (Greenstone and Hanna, 2014; Landrigan, 2017; European Commission, 2019a). Although cars have allowed urban dwellers to escape the overcrowding and high urban rents in the inner city, they require an immense amount of space. This has resulted in urban sprawl and its associated externalities, including social exclusion, habitat loss, and pollution (Glaeser and Kahn, 2004; Nechyba and Walsh, 2004; Su and DeSalvo, 2008; Brueckner and Helsley, 2011). Finally, traffic accidents cause over 25,000 deaths and more than one million injuries annually in the EU, resulting in an estimated €280 billion of societal costs (European Commission, 2019a).

While these adverse outcomes are tragic and partially avoidable, the fact that cars have been adopted *en mass* around the world signals that the net welfare gains to society must be positive. We can travel faster, more conveniently, and cheaper than ever before in history. However, our experience with the car should serve as a warning for future transport technologies. As we have seen with the push-back against congestion tolls and parking prices, it is far more difficult to start charging for things that people used to get for free (Shoup, 2005; Anas and Lindsey, 2011; De Borger and Proost, 2012). Therefore, governments should take a proactive stance towards anticipating and implementing policies ahead of time to mitigate adverse transport externalities.

This leads us to where we are today. Although the rise of smartphones over the last two decades has brought about a flurry of innovation in 'last-mile' transportation technologies such as shared taxi's, industry leaders, politicians, and academics claim that we are on the cusp of the next transport revolution: the automated vehicle. Automated vehicles (AVs) are considered to be a disruptive technology with the potential to transform our concept of mobility. By utilizing sensory equipment to map and detect their surroundings, AVs are expected to be able to *operate without human intervention*.[3] This appears to be increasingly feasible as demonstrated by industry commitments, technological advancements, political support, and cultural attitudes (Fagnant and Kockelman, 2015; Milakis et al., 2017; Soteropoulos et al., 2019).

The goal of this dissertation is to "study the present in the light of the past for the purposes of the future" (Keynes, 1924, pg.322). I apply a quantitative approach, using data sources that are currently available, to study the effects of private vehicles

[2]Average speed on the road network in a global sample of 26 large cities from the UITP (2015) database was only 29 km/hr in 2012.

[3]We define an AV as a vehicle that can perform all driving tasks without requiring a human driver. This corresponds to the level 5 AV definition by the European Parliamentary Research Service (2016).

on urban economic outcomes. These empirical analyses focus on market and government failures in the ownership and use of private vehicles with a purpose to inform urban and transport policy. In the spirit of Keynes, these insights are then applied to provide policy makers with a glimpse into what an AV future might look like – absent policy intervention.

In cities, parking occupies a large share of land and is often provided to residents and visitors at prices below the market rate. According to economic theory, this causes excess car ownership and use however, we lack well defined quantitative estimates of these effects. Chapter 2 studies the effect of a large citywide increase in hourly on-street parking prices on parking and traffic demand in Amsterdam. Chapter 3 then focuses specifically on residents and examines to what extent residential parking prices effect car ownership decisions. In both chapters, we aim to draw policy conclusions on the quantitative impact of parking prices on car use to gauge the implications of underpriced parking.

Vehicle accidents account for over €200 billion in annual costs within the EU, so it comes as no surprise that reducing the number of road accidents is a major policy goal. All EU countries currently ban phone use by drivers however, surveys indicate that 60% – 70% of drivers admit to "sometimes" using their mobile phone while observational studies find that 1% – 11% of drivers were physically observed to be on the phone at any given time (European Road Safety Observatory, 2015). Chapter 4 studies how the rise in smartphone use over the past decade has impacted road safety. This is essential to understand because although it is illegal, enforcement is difficult. A quantitative estimate of the number of accidents caused by smartphone distractions provides valuable information to policy makers on the size of the issue, and in turn, how serious the regulatory response should be.

The urban economics literature has demonstrated that highways have had a substantial impact on decentralisation over the twentieth century (Baum-Snow, 2007, 2010; Garcia-López et al., 2015; Baum-Snow et al., 2017; Levkovich et al., 2017). However, highways only explain a portion of car-induced decentralisation. Various other policies and implicit subsidies such as free parking, low fuel taxes, and the absence of road pricing, are likely to impact mode choice and thereby urban form. This is important because the urban spatial structure changes *slowly*, so transport policies can have long-lasting and irreversible impacts on urban economic outcomes. Therefore, Chapter 5 uses car ownership as a comprehensive measure to study the overall long-term effect of cars on urban density. This has important implications for vehicle taxation and car ownership growth in middle-income countries.

In each chapter, the empirical estimates are applied to predict the order of magnitude impacts of AVs over space, under the assumption that policy and the behavioural phenomena studied remain largely unchanged in the future. AVs are expected to impact urban mobility along four major dimensions: parking, safety, cost, and convenience (Fagnant and Kockelman, 2015). Parking patterns are likely to change as AVs can self-park in areas with lower parking prices. AVs are also expected to be safer than human drivers as they are less likely to get distracted, drunk, or sleepy. Finally, passengers taking an AV will be able to participate in other, leisurely or productive, activities such as sleeping, reading, and working, instead of driving, without having to pay a driver.

These mobility changes are expected to result in improvements to accessibility and safety, increases in car demand, and a redistribution of people and jobs over space (Meyer et al., 2017; Gelauff et al., 2019). Chapters 2 and 3 examine how lower parking prices are expected to impact traffic and vehicle demand within cities. Chapter 4 provides an indication for the potential safety benefits from AVs due to fewer smartphone distractions. Chapter 5 then examines how increases in vehicle access and demand, due to cheaper and more comfortable car travel, are expected to impact urban population density in the long-run.

Chapter 6 concludes with an outline of the main results and a discussion of the key implications for urban transport policy and AVs.

2

Hourly parking prices and traffic

2.1 Introduction

Parking prices are a widely accepted policy tool to manage parking and traffic demand in cities. The theoretical economic literature has extensively studied parking policy as a second-best alternative to tackle traffic externalities by reducing the number of car trips in urban areas.[4] In light of the technical and political challenges of implementing road pricing, parking policies have come under renewed interest because they already exist in many cities and therefore extending these policies may be more feasible (Small and Verhoef, 2007). Nevertheless, the empirical literature has yet to confirm or refute the effectiveness of parking prices in reducing traffic demand.

This chapter is based on join work with Hans Koster, Leonardo Nunes, and Jos van Ommeren. The authors would like to thank Lea Bou, Joris Klingen, Maurice de Kleijn, Rossy Nguyen, Giles Ostermeijer, Kalani Ostermeijer, Fillipo Tassinari, Erik Verhoef, and online conference and seminar participants at Eureka, ITEA, and the UEA. We would also like to thank Abdel En-Nali, Barry Ubbels, Leon Deckers, Marco van Leeuwen, Martijn Kobus, and Rutger van Zaanten at the Gemeente Amsterdam for many enlightening discussions and granting access to on-street parking and traffic data, and Ron Peerenboom at the Ministry of Infrastructure for providing the off-street parking data.

[4]See e.g. Anderson and de Palma (2004), Albert and Mahalel (2006), Arnott and Inci (2006), Arnott and Rowse (2009), Calthrop and Proost (2006), Fosgerau and De Palma (2013), and Arnott et al. (2015).

In this paper, we aim to fill this gap by examining to what extent hourly on-street parking prices are an effective second-best policy tool to mitigate urban traffic externalities by reducing citywide road traffic. We focus on the city of Amsterdam, where on-street prices are high and comparable to off-street prices. We use information on on-street parking, off-street parking, and traffic flow for a period during which on-street parking prices were suddenly and strongly increased throughout the city.

To estimate the causal effect of the price increase on parking demand and traffic flow, we apply an event study approach, where we examine changes in parking demand before and after the policy change, controlling for seasonality, location fixed effects, and time trends. Our key identification assumption is that the timing of the policy is random and that in the absence of the policy, parking as well as traffic flow should follow similar trends in the pre and post period, for which we provide convincing graphical evidence. Alternatively, we exploit spatio-temporal variation in parking prices, which identifies local parking demand elasticities to support our citywide estimates.

We first show that the price effect on on-street parking demand is large and robust. The increase in parking prices due to the policy caused overall hourly on-street parking demand to decline by around 17% and the number of arrivals to decline by 9%, corresponding to a citywide price elasticity of demand of -0.37 and -0.19, respectively. We also find negative, but much smaller, effects in the (commercial) off-street parking market, as off-street providers increased prices as a reaction to the policy, but to a lesser extent. Taking into account that about one quarter of car trips in Amsterdam use hourly on-street parking, this implies that the policy decreased *citywide* traffic flow by around 2.5%. Furthermore, we find that the total effect on parking arrivals and exits is over twice as large during afternoon peak hour traffic as compared to the morning peak. These results are confirmed using traffic counts from road loop data, where we find a subsequent average reduction in traffic flows of around 2% – 3%, and larger effects in the afternoon.

These findings are important to understand the extent to which prices reduce parking demand and traffic at the city level. One straightforward implication is that parking prices reduce overall traffic flow and thereby serve as a second-best congestion and environmental policy. Interestingly, we show that even though parking prices were not differentiated within the day, the policy had larger effects on traffic during the evening peak hours because of heterogeneity in parking demand within the day. Our estimates are also relevant for cities aiming to convert on-street parking into alternative uses, such as parks, cycling lanes, and restaurants, without causing additional externalities from cruising or additional costs from building new off-street capacity.

Our paper relates to three strands of literature. First, our paper relates to the empirical literature studying the effects of prices on demand. Lehner and Peer (2019) present a meta-analysis on the price elasticity of parking.[5] Second, our paper relates to a large theoretical literature, which emphasises the importance of using parking prices to reduce congestion (Albert and Mahalel, 2006; Arnott and Inci, 2006; Shoup, 2006; Arnott and Rowse, 2009; Arnott and Inci, 2010; Fosgerau and De Palma, 2013; Arnott et al., 2015). Third, our paper links to the literature on second-best congestion policies. This includes public transport subsidies (Anderson, 2014), licence plate restrictions (Davis, 2008; Kreindler, 2016), and HOV lanes (Bento et al., 2014; Hanna et al., 2017). Most closely related to our paper, Krishnamurthy and Ngo (2020) study the effects of a local parking policy on traffic flow and find that the introduction of a dynamic pricing scheme in San Francisco resulted in 6% lower vehicle counts in treated areas. However, the latter study is silent on the effect at the city level.

Our study contributes to the existing literature in three ways. First, we estimate on-street as well as off-street parking demand functions for the whole city of Amsterdam, where we exploit a substantial increase in the hourly price for on-street parking for essentially the whole city. In the parking demand literature, typically a local price change is investigated, either for a specific parking garage or parking zone (Kelly and Clinch, 2009; Van Ommeren and Wentink, 2012; Krishnamurthy and Ngo, 2020). Effects on local parking demand are then a combination of a reduction in car use and substitution to other locations.[6] Using *all* on-street parking data *and* a representative sample of commercial off-street garages for essentially the whole city offers the key advantage that we are able to address substitution over space to other locations within the city.[7] The policy we examine increased average prices by 66%, from €2.55 to €4.22 per hour, so the price increase was not only large in relative terms, but also in absolute terms.

Second, our study presents a significant improvement in data quality compared to the previous literature (Lehner and Peer, 2019). We use administrative micro-data from over 70 million parking transactions at more than 3,000 parking meters and 60 visitor permit zones throughout the city, which represents the complete hourly on-street parking market. These data allow us to estimate citywide parking elasticities and distinguish between the extensive (parking arrivals) and intensive margin (park-

[5]Notable contributions include Kelly and Clinch (2009) (on-street parking demand), Pierce and Shoup (2013), Ottosson et al. (2013), Chatman and Manville (2014) (on-street parking occupancy), and De Groote et al. (2019) (off-street parking demand).

[6]For example, it is unclear whether the effects on traffic flows found in Krishnamurthy and Ngo (2020) were redirected to other locations in the city that were not part of the pilot programme.

[7]For larger cities it is unlikely that many motorists decrease parking demand within the city by increasing parking demand just outside the city. Even if this would be the case the reduction in citywide traffic flow is still captured by the reduction in parking demand in the city as drivers do not enter the city.

ing duration). This is crucial, as information on parking arrivals allows us to gauge the effects on traffic flow. Furthermore, we also have a representative sample on off-street garages which indicates that off-street prices in Amsterdam also increased in response to the policy and rules out substitution to off-street parking.

Third, using traffic flow data from road loops, we explicitly estimate the effect of the citywide increase in parking prices on traffic flow within the city, largely confirming the parking results.

The rest of this paper is structured as follows. Section 4.2 describes the policy context and data, Section 4.4 explains the methods employed, and Section 4.5 discusses our results, robustness checks, and implications. Finally, Section 4.6 concludes.

2.2 Data and context

2.2.1 Context

Amsterdam is a historic European city, characterized by narrow one-way streets and by a transportation system that offers many modal alternatives to travellers. In 2017, auto travel represented 27% of trips, while cycling, walking, public transport, and scooters, each accounted for 26%, 19%, 26%, and 2%, respectively (Gemeente Amsterdam, 2019). About half of all car trips, excluding those made by residential parking permit holders, are made by non-residents.[8]

Figure 2.1 illustrates a map of the Amsterdam municipal area and shows the major transport and parking infrastructure. Travelling from one side of the city to the other by car is fastest and most convenient via the A10 ring road, so most cars do not travel through the city unless they are going to a destination within the ring road. The river IJ cuts the city in two parts. Access from the South to the North of Amsterdam by car is only possible via three tunnels, one to the West and two to the East of the central train station.[9] The dark gray area indicates the paid parking area which represents 63% of the total on-street parking supply in Amsterdam. Peripheral areas without paid parking are predominantly residential suburban or industrial, are generally not well connected to tram, metro or bus lines, and are generally not considered as a viable substitute for motorists with a destination in the paid parking area. There are

[8]Trips to and from home by Amsterdam residents account for around 30% of all car trips, however these do not end in (hourly) paid parking.

[9]For non-motorized transport, the only way to get to the North side is by ferry from Amsterdam Central Station, or by taking the North-South metro line that was opened in July 2018.

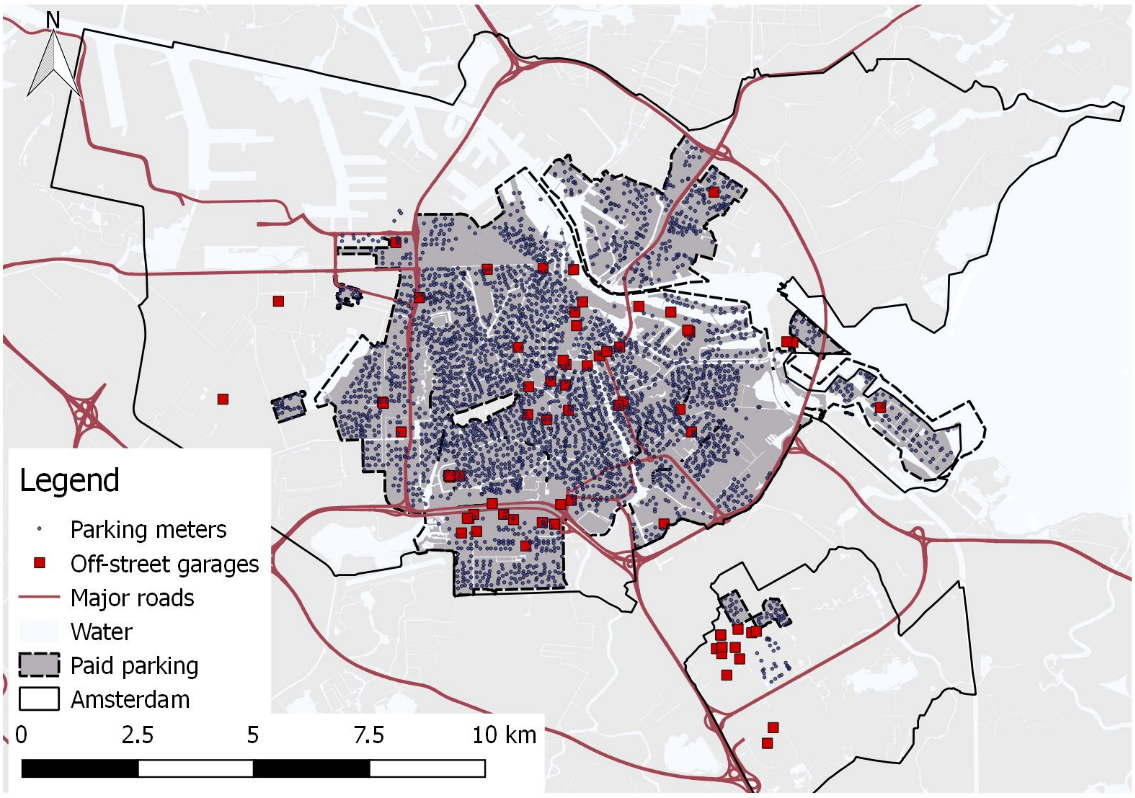


Figure 2.1: Major transport and parking infrastructure in Amsterdam.

three clusters of off-street parking garages. The first cluster, which contains the majority of garages, are located around the city centre and are small in terms of capacity. The other two clusters are around the South Axis business park and the Bijlmer ArenA towards the South East, which tend to have larger capacities.

2.2.2 Parking policy in Amsterdam

In this section we briefly describe the policy context and main impacts of the policy we analyse (for a more detailed overview, please see Appendix 1). In Amsterdam on-street parking is accessible to drivers who pay hourly rates as well as residents using residential permits, close to their home.[10] Around one third of motorists (excluding residents parking with a permit) use off-street commercial parking garages. These garages are mainly provided by private operators and charge slightly higher prices

[10]This is in contrast to countries such as the UK, where most cities have 'residential parking only' areas. Residential permits are only valid in a residential permit zone.

as compared to nearby on-street parking.

In May of 2018, the city of Amsterdam committed to a mobility agenda to prioritise cycling and pedestrian transport, while reducing car use in the city (Gemeente Amsterdam, 2018a, pg.47). Following a decade of constant on-street parking prices, the new coalition government, headed by the Green party, mandated a parking price increase for (hourly) on-street parking and the conversion of freed on-street parking supply to other uses. By late October 2018, it was announced that (hourly) prices were to be raised throughout the city effective Sunday April 14, 2019 (week 16).

A map of Amsterdam municipality illustrating the spatial extent of the paid parking area and parking prices per zone before and after the policy can be seen in Figure 2.2. There are eight price zones that differ in their hourly prices. Prices are the highest in the historical city centre and fall with distance to the centre. Price increases were large in both relative and absolute terms (see Figure 2.A.4 in Appendix A). Average hourly on-street prices (weighted by the number of arrivals per area) increased by €1.67, or 66%, from €2.55 to €4.22. In the historic city centre, hourly prices went up from €5.00 to €7.50, making Amsterdam the most expensive city for on-street parking in the world (Parkopedia, 2019).

Price increases were implemented in every parking zone except for three non-central industrial zones with a time limit of three hours and a few streets with a time limit of one hour, priced at €0.10 and intended to be used for shopping (see Figure 2.A.1 in Appendix A).[11] The largest relative price increase occurred just outside the city centre where prices doubled from €3.00 to €6.00. The smallest relative price increase occurred in northern areas and a few peripheral areas of the city where price increases were negligible.

The new policy did not alter paid parking hours. Paid parking hours, which vary by zone, start at 9:00 and end between 19:00–23:59. For the majority of parking areas within the ring road, parking hours end after 21:00. Furthermore, as shown in Figure 2.2, the policy did not affect the total paid parking area, but it slightly changed the delineation of certain parking zones within this area.[12]

Alongside the price increase, the municipality aims to gradually reduce the supply of on-street parking in areas where parking pressure was relieved due to the price increase. In 2019, 1,141 parking places were redeveloped into public spaces such as park benches, playgrounds, and bicycle parking (Gemeente Amsterdam, 2020). The

[11] Reducing parking demand through time limits is common in North American and Australian cities, but is relatively rare in Europe.

[12] The policy also expanded a visitor permit scheme which accounts for a small share of on-street parking demand (1.57%). In our analysis, visitor permits are included.

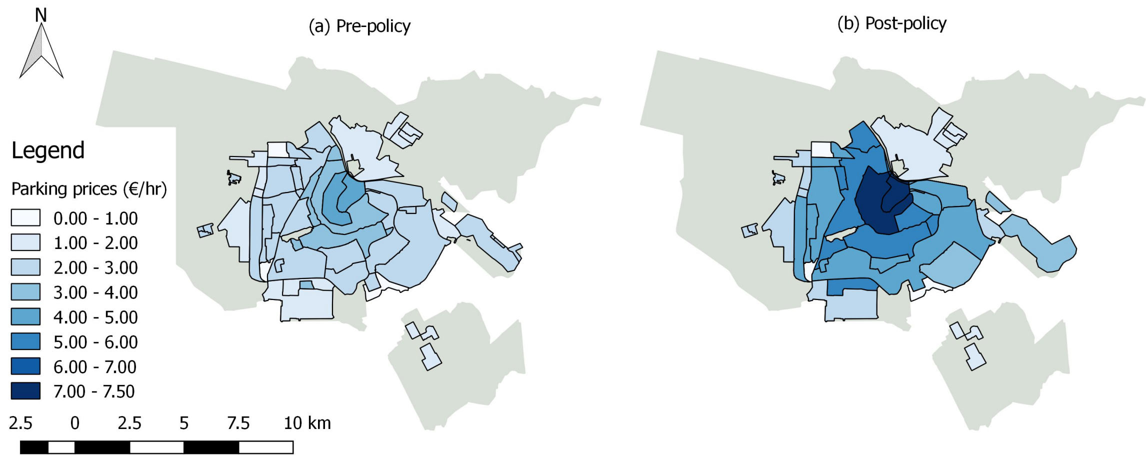


Figure 2.2: Hourly parking prices pre and post policy.

reduction was gradual, relatively uniform over space, and represents less than one percent of the total paid on-street parking supply. Hence, this is unlikely to be a confounding factor in evaluating the effect of the price increases, because the reduction in supply was a *response* to lower demand, and the reduction is only a fraction of the decrease in demand implied by our results. Hence, the reduction in supply is unlikely to have contributed to increased cruising and therefore to reduced parking demand.

Hourly prices at commercial off-street garages (weighted by garage capacity) were, on average, almost 30% higher than nearby on-street prices before the policy (€3.33 and €2.57, respectively), but after the policy the difference was less than 10% (€4.37 and €4.01, respectively).[13] So prices for off-street parking garages increased substantially (by 19% – 31%), but less than the on-street prices close to these garages (which went up by 56%).[14] While cruising for parking is limited compared to other major cities, the reduction in the difference between on-street and off-street parking prices suggests a (small) reduction in the level of cruising costs (Arnott et al., 2015).

[13]We define 'nearby' as parking meters within a 500 m buffer around each off-street garage and calculate the average price of these parking meters. Note that off-street *day* prices are generally lower than for on-street parking (before and after the policy) so the overall price difference is less than indicated in the main text.

[14]In our full sample it is 31%. For some garages we do not observe prices pre-policy. Excluding these garages results in an increase of 19%.

2.2.3 Parking data

2.2.3.1 On-street parking

The on-street parking analysis is based on administrative data of 87.51 million unique on-street parking transactions from 2017 to 2019, provided by the municipality of Amsterdam.[15] This micro dataset contains information about the start and end time of each transaction, as well as other transaction attributes such as the total price charged, the parking meter, as well as the type of use and method of payment.[16] We exclude 3.51% transactions which are used for special purposes, such as handicapped parking and long term construction work.

Motorists are required to pay at the closest available parking meter, but the majority use mobile phone apps (76%). The latter allows for flexibility in terms of duration as compared to physically paying at the machine, where duration must be chosen beforehand. On-street parking is also possible via visitor permits, which are available through residents. These permits offer a discount of between 50% – 75% on the hourly rates for up to 40 hours per residential household per month, and must be activated via an online web application.[17] Each transaction is tied to a vehicle number plate and enforcement is performed using a car equipped with cameras, therefore infraction is difficult, however illegal parking still accounts for over 2% of arrivals (Egis Group, 2019).[18]

We exclude 3.85% of transactions shorter than five minutes and 0.01% of transactions longer than one week as they are likely to be the result of human and machine errors. Furthermore, we exclude transactions on Sunday (3.3%) as parking hours and rates differ compared with the rest of the week and on-street parking tends to be free. Finally, there was a large expansion of the parking area in the North of Amsterdam on July 1, 2018, corresponding to the introduction of a new metro line. Because the North is geographically separated from the main area of Amsterdam by the IJ river and faces different trends, we exclude this area (7.5% transactions) and perform a sensitivity check where we include these parking areas, while controlling for area

[15] For 2017, observations for 9 weeks are missing.

[16] Parking meters are close together. The median distance between a parking meter and the next closest parking meter is 69 meters.

[17] Transactions have a visitor parking zone as a spatial identifier which contains about 42 parking meters per zone.

[18] In 2017, the municipality issued 780,000 fines, which corresponds to around 2% of arrivals (Parool, 2019). This is a lower bound of the prevalence of illegal parking as it is inevitable that some infractions go undetected. Nevertheless, the effect on arrivals is likely to be smaller because many infractions occur due to underpayment. If no infraction is detected, the number plate data is removed on privacy grounds.

specific time trends.

After these selections we are left with 67.13 million parking transactions, of which 98.7% pay the full price and park for an average duration (weighted by the number of arrivals) of 2.4 hours, while 1.3% use visitor permits with a slightly higher average (weighted) duration of 3 hours. Using these micro data we calculate daily parking demand per area resulting in a panel of 2.71 million daily observations.[19] For motorists that pay the full price we know the parking meter and for those that use visitor permits, we know the visitor parking zone. In total we have 3,238 parking meters and 67 visitor parking zones.[20]

Daily parking demand per area is measured in three ways: volume (total hours parked), the number of arrivals, and the mean duration of arrivals. Most transactions (96.2%) start and end on the same day, therefore volume is (approximately) equal to the product of arrivals and duration at the daily level. Based on hourly data from the nearest weather station, obtained from KNMI,[21] average daily temperature (°C), windspeed (kmph), and a dummy for rain and temperatures below 0 °C between 08:00 – 20:00 are added. We also add public holidays and school holidays as additional controls as vacation times change by region from year to year.

2.2.3.2 Off-street parking

We observe the location and hourly prices for all 70 off-street garages. Garages have an average capacity of 455 spaces. The municipality of Amsterdam owns 40% of garage capacity and charges market prices, so these garages are defined as commercial.

For a (representative) sample of 27 garages (out of 70), we have occupancy data based on API requests to dynamic parking information systems which allow us to calculate hourly parking volume.[22] There are two limitations of these data. First, we do not

[19] We trim outliers with volume ≥ 1000 hours, duration ≥ 24 hours, and arrivals and exits ≥ 500 cars (0.2% of observations). See Section 1 in Appendix A for a detailed description of the aggregation process.

[20] Most parking meters are active throughout the entire period. However, 73 parking areas were either defective for a period of at least one month or were added/removed during the study period. These areas correspond to 0.56% total arrivals, are evenly spread throughout the city, and are included in the analysis.

[21] Data from the Schipol weather station is used, located 12 km away from the city center. The KNMI is the Dutch National Weather Institute.

[22] See Figure 2.A.1 in Appendix A for spatial distribution of garages in the sample. Garage occupancy is observed every 2 minutes.

observe the number of off-street garage arrivals (or exits) in which we are interested to gauge the policy effect on traffic flow. As will be explained in detail later on, the estimate of the policy effect on volume can be used to bound the effect on the number of arrivals. Second, we only observe garage data after July in 2018. This means that we have less information on longer term (pre)trends, however given a sudden change in on-street prices, we still expect to be able to detect changes around the policy window.

Our aggregated daily dataset consists of 10,395 daily parking volume observations for 16 commercial garages, covering 31% of total commercial off-street capacity, and 7 P&R facilities, covering 63% of P&R capacity, between July 4, 2018 and February 29, 2020.[23] Average hourly prices at commercial garages increased by 23%, while prices at P&R facilities, which charge cheap daily rates of €1, conditional on drivers parking after 10:00 and demonstrating a valid public transport ticket to and from the city centre, did not change.[24]

2.2.4 Traffic data

We further obtain hourly flow data from primary (non-highway) roads measured using induction loops at various points within the city for the years 2018 and 2019 from the municipality of Amsterdam. Our aggregated data consists of 12,696 daily observations for a total of 31 loops where traffic flow are collected, where each loop represents one flow direction.[25]

2.2.5 Trends

Figure 2.3 shows that on-street parking volume and the number of arrivals both exhibit a slight positive linear growth rate, as indicated by the black linear fit, over the period before the introduction of the policy, while duration is constant. There is a sharp decline in parking demand at the beginning of the policy, followed by a con-

[23]We exclude 0.75% of observations for unrealistic outliers and 5.18% of observations with incomplete hourly data. We drop 4 garages because of missing data and select the period until March 1, 2020 due to COVID-19 lock-down measures.

[24]Other drivers pay hourly rates that are similar to on-street prices. Based on other monthly data from the municipality, we can calculate that there are almost 2,000 daily P&R arrivals (80% of these exit the same day).

[25]Several loops have defective measurements and experienced nearby road works over the period of study. We pre-select locations for which we have consistent observations over the period of analysis.

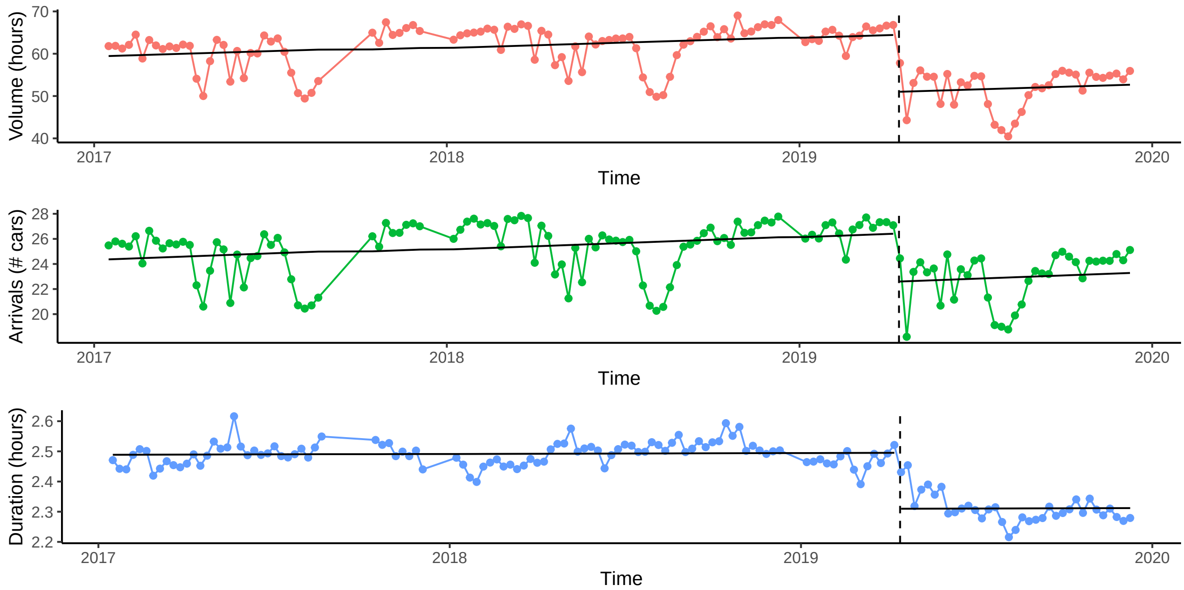


Figure 2.3: Mean daily on-street parking demand per area.

tinuation of the linear trend until the end of 2019. It can also be seen that there is a dip in the volume and number of arrivals around the school summer holidays and there is variation in the number of arrivals around April and May, which is the result of a large number of public and school holidays falling in this period (7 out of of 11 mandatory public holidays fall in April or May).

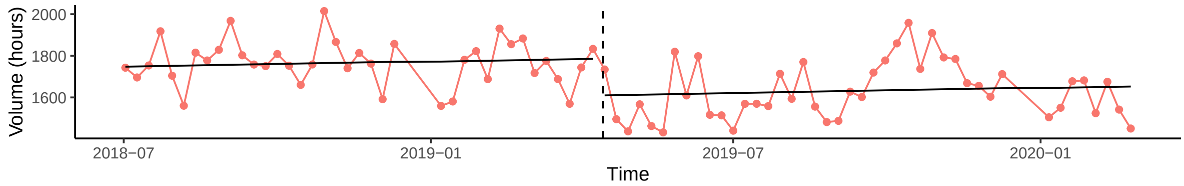
Figure 2.B.5 in Appendix B illustrates trends in traffic flow over time.[26] Traffic flow appears to follow similar patterns over time as parking demand with dips in the summer period and more fluctuation around holidays.

Figure 2.4 shows that commercial off-street parking volume is constant pre-policy, while P&R volume is falling. At the beginning of the policy, there appears to be a slight drop in commercial off-street parking and an increase in P&R demand.

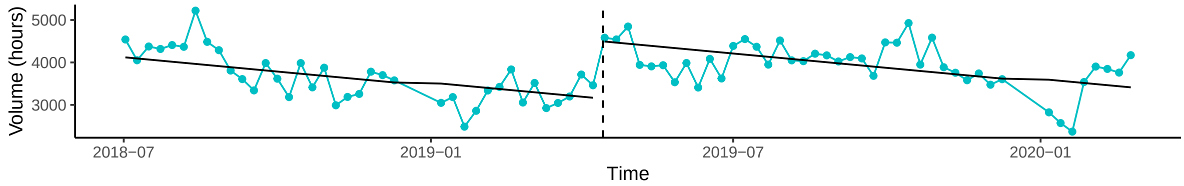
2.2.6 Descriptive statistics

Table 2.1 presents the descriptive statistics. Panel A shows that there are around 25 daily arrivals per parking area and the mean duration is 2.5 hours, the product of which approximately equals the daily parking volume per area, which is 61

[26] As we do not have a balanced panel, the data is demeaned per loop-direction to ensure comparability over time.



(a) Commercial



(b) P&R

Figure 2.4: Mean daily off-street parking demand per garage.

hours parked.[27] Panel B shows that the average daily volume at off-street commercial garages is about 1,700 hours while P&R facilities have slightly over double the daily volume. Panel C indicates that average daily traffic flow is about 9,000 cars per loop-direction.

In Appendix 5 we present histograms of the key variables and the distribution within the day. Figure 2.A.5 shows that on-street parking volume peaks between 10:00 and 15:00 and gradually falls until midnight. Average duration is constant during the day and becomes slightly longer in the evening. Arrivals peak at 09:00 when paid parking starts and is relatively constant until 18:00, after which arrivals begin to fall. There are few exits before 10:00 and peak around 15:00. Figure 2.A.8 indicates that off-street parking volume follows a similar hourly distribution and accounts for around one third of total paid (hourly) parking demand. Finally, Figure 2.A.9 illustrates that traffic flow peaks at 08:00 and 17:00, but does not fluctuate a great deal over the day.

2.3 Empirical methods

Our aim is to estimate the causal effect of parking policy on parking demand and traffic flow at the city level. The policy implied higher on-street and off-street prices as the policy induced commercial off-street providers to increase garage prices. The causal effect of the policy on parking demand is estimated using an event study ap-

[27]There are slightly fewer observations for duration as a small proportion (2.02%) of parking areas face no arrivals on a given day.

Table 2.1: Descriptive statistics

Statistic	N	Mean	St. Dev.	Min	Max
Panel A: On-street parking					
Volume (hours)	2,710,535	59.37	73.99	0.00	999.97
Arrivals (# cars)	2,710,535	24.76	27.66	0.00	439.00
Duration (mean hours)	2,655,737	2.52	1.43	0.08	23.99
Panel B: Off-street parking					
Volume commercial (hours)	6,514	1,704.61	1,242.71	0.00	6,181.83
Volume P&R (hours)	2,797	3,863.66	1,564.74	0.00	11,288.00
Panel C: Traffic flow					
Flow (# cars)	12,696	8,863.69	3,786.36	1,994	26,060

proach, controlling for seasonality, area fixed effects, and time trends. Our key identification assumption is that the timing of the policy is random and that in the absence of the policy, parking demand would have followed a similar trend in the pre and post period, for which we provide convincing graphical evidence (see Section 2.4.1.1). We first introduce the econometric model, and subsequently discuss how we deal with various endogeneity issues that arise in our setting.

2.3.1 On-street parking

We first aim to examine to what extent the policy impacted on-street parking demand using only temporal variation from the introduction of the parking policy. Hence, our dependent variable of interest is parking demand, which we define as D_{it}, for each parking area i at day t. Parking demand is measured in three ways: volume (i.e. 'total demand'), arrivals (i.e. the 'extensive margin'), and average duration (i.e. the 'intensive margin'). Parking areas $i = 1, ..., n, n+1, ..., N$ refer to n parking meters and $N - n$ visitor permit areas. We consider the following exponential mean function:

$$E[D_{it}] = \exp(\beta T_t + \phi_i + \kappa \mathbf{S}_t + \tau \mathbf{W}_t + L(t)), \tag{2.1}$$

where $E[D_{it}]$ denotes the expected demand and the policy effect is denoted by $T_t = \{P_t, \log(\bar{p}_t)\}$.[28] P_t is a dummy equal to one after the policy was introduced and $\log(\bar{p}_t)$

[28]This is estimated using a Poisson Quasi-Maximum likelihood estimator. The exponential mean model has an advantage over log transformations because it allows for zero counts and is insensitive to the level of spatial aggregation. In Table 2.B.5 of Appendix B we show that our estimates are conservative compared to the log transformation.

equals the natural logarithm of the average price level over the city at time t. Therefore β represents the semi-elasticity of *citywide* parking demand with respect to the policy and citywide average prices, respectively. Standard errors are clustered at the time-invariant level of a parking area.

We have a slightly unbalanced panel as new parking meters have been added over time. Therefore we include parking area fixed effects, ϕ_i, which capture time-invariant characteristics related to demand, such as the availability of substitutes (i.e. public transport) and the attractiveness of the area (i.e. availability of shops and firms), and parking supply (i.e. parking demand by residents with a permit). Parking demand fluctuates over the year due to time varying demand factors, such as holidays, weekends, and weather conditions. While this is unlikely to affect the consistency of our estimates, we control for temporal fluctuations in demand to improve efficiency by including fixed effects, represented by $\mathbf{S}_t$, for day-of-week, week-of-year, public holidays, school holidays, and a vector of weather controls $\mathbf{W}_t$.[29] Finally, time trends are an important confounding factor as our key identifying assumption relies on the correct specification of the time trend. Trends in parking demand appear to indicate a small, positive, linear time trend (see Figure 2.3), therefore in our main specification we include a linear time trend, $L(t)$, and perform various sensitivity checks where we include parking area specific trends and include higher order polynomials. Lastly, in spirit of a Regression Discontinuity Design, we also perform the analysis over a shorter time window of one, two and three-months pre-post, to abstract from longer term trends.

2.3.2 Off-street parking and overall parking demand

One issue with equation (2.1) is that the policy may induce motorists to substitute to off-street garages (including P&R), which would result in an overestimate of the policy effect on traffic. As mentioned in Section 4.3, prices at off-street garages also increased and parking off-street remained more expensive than on-street, however off-street parking became relatively cheaper. Furthermore, the price for P&R garages did not change, so it became more attractive for drivers to park in the *outskirts* of the city and to take public transport *into* the city.

To get an understanding of the overall effect on (hourly) paid parking demand, we first estimate the effect of the policy on commercial off-street and P&R parking de-

[29]Weather controls include average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 08:00 – 20:00. These controls potentially increase the efficiency of the estimates. They will also improve the consistency as the price increase was not on the first of January, so it is partially correlated to seasonal factors.

mand separately. This provides an indication of how increasing hourly on-street parking prices affects off-street parking demand. To estimate the overall effect of the parking policy on the entire market for hourly parking in Amsterdam, we include all hourly parking (including on-street parking) into one combined estimate. We only have parking volume data for a sample of garages over a shorter period, so we weight each off-street and P&R garage by the sum of total capacity divided by the sum of capacity for garages we observe.[30] Under the assumption that the policy effect on garages in our sample is representative, the combined regression estimates the overall effect of the parking policy on the entire market for hourly parking in Amsterdam.[31]

Another potential issue with equation (2.1) is that the policy may induce drivers to park (for free) outside the paid parking areas and commute into the city using public transport. Although we cannot measure this effect, it is likely to be small for three reasons. First, the majority of motorists park for a short period of time (64% park for less than two hours), so the additional time cost of parking outside the paid area frequently exceeds the duration of the activity. Second, it costs around €4.00 for a return trip by public transport from outside the paid parking area to the city centre, so the monetary opportunity costs are substantial. Third, motorists are unlikely to significantly contribute to traffic *within* the paid parking area, which we are mainly interested in.

Finally, if the policy induces more illegal parking our estimates of parking demand may be downwards biased (Yang and Qian, 2017). This can take the form of drivers that leave after the end time stated in the transaction data, or that simply park illegally (without paying). In Amsterdam, this is unlikely to be a large issue as enforcement is strong and technologically advanced.[32]

2.3.3 Temporal and spatial variation in on-street prices

On-street parking price increases varied throughout the city (see Figure 2.A.4 in Appendix A), so we expect drivers to react to spatial differences in prices within the city to varying degrees. Therefore our second identification strategy exploits both temporal and spatial variation from changes in parking prices as an internal consistency check to verify that parking price changes are driving our results, rather than other

[30] In effect, commercial garages get a weight of 3.2 and P&R facilities get a weight of 1.6 each.

[31] In Amsterdam, in contrast to many other cities around the world, (free) parking offered by retail companies (e.g. supermarkets and malls) is negligible, so traffic related to shopping is included in hourly parking demand measures.

[32] Illegal parking accounts for about 2% of arrivals (see Section 4.2).

confounding factors. We estimate a similar equation as above:

$$E[D_{it}] = \exp(\beta \log(p_{it}) + \phi_i + \kappa \mathbf{S}_t + \tau \mathbf{W}_t + L(t)), \tag{2.2}$$

where the policy effect is now captured by $\log(p_{it})$, which represents the natural log of on-street parking prices for parking area i at time t. Here, β represents the elasticity of parking outcomes with respect to parking prices *at a specific parking location*, but does not provide an unbiased estimate of the *citywide* parking price effect, estimated in equation (2.1), as it captures spatial substitution within the city. For instance, if prices in one area increase, while in a neighbouring area prices stay the same, we might expect drivers to substitute to these areas, in which case demand shifts to another location *within* the city, but overall *citywide* parking and traffic demand remains unchanged. So, prices in neighbouring areas may affect parking demand at location i.

Therefore, we estimate two variants of equation (2.2). First, parking areas far from boundaries are likely to have less substitution due to long walking distances, so we examine whether excluding parking areas close to the border of parking rate zones affects our estimates. Second, we calculate the difference in parking prices between parking area i and neighbouring areas, $j \neq i$, by calculating the average price of parking meters within a 500 meter buffer. We then include this variable non-linearly into equation (2.2) to check whether differences in neighbouring prices influence the local estimates.[33]

We also apply a different empirical strategy where we estimate a variant of equation (2.2) that exploits spatio-temporal variation from changes in price differences between locations using a two-way fixed effects model. Therefore, we include parking area fixed effects, ϕ_i, *and* day fixed effects, γ_t, leading to the following regression equation:

$$E[D_{it}] = \exp(\beta \log(p_{it}) + \phi_i + \gamma_t), \tag{2.3}$$

where all time-varying covariates are absorbed by the day fixed effect and β represents a difference-in-differences estimator, where treatment is continuous and is determined by the intensity of relative price changes.[34] Therefore, our alternative identifying assumption is that in the absence of the policy, areas with larger relative

[33] According to Van der Waerden et al. (2017), the maximum distance car drivers are willing to walk is about 50 m for work, 100 m for weekly shopping, and above 500 m for non-weekly shopping. As we focus on weekdays and Amsterdam has considerably larger spatial differences in parking fees we apply a slightly conservative approach with 500 m.

[34] In effect, we compare changes in parking demand for areas that experience, for example, a 50% increase in prices with areas that experience an 80% increase in prices and identify β based on the difference in changes (i.e. $80\% - 50\% = 30\%$).

price increases should face similar changes in parking demand as compared to areas with smaller changes.

2.3.4 Traffic effects

We focus on paid parking arrivals, which capture a substantial share of total traffic, but far from all. Total traffic also relates to trips by residents using residential parking permits or private parking, by commuters who predominantly use (free) employer parking, by public and shared transport vehicles, such as buses and taxi's, and by delivery vehicles. Therefore, we also examine to what extent the policy impacted traffic outcomes in the city using traffic flow data in order to validate our estimates of parking demand. We define $\log(F_{it})$ as the natural logarithm of total traffic flows (measured by cars per day) for each measurement area i at day t and estimate a model with the same set of controls as in equation (2.1):

$$\log(F_{it}) = \beta P_t + \phi_i + \kappa \mathbf{S}_t + \tau \mathbf{W}_t + L(t) + \epsilon_{it}, \tag{2.4}$$

where β represents the semi-elasticity of citywide traffic flow with respect to the policy. The (loop-direction) area fixed effect, ϕ_i, captures time-invariant characteristics of the traffic measurement location, such as the road type, route direction, and proximity to the highway. As in (2.1), our identification strategy exploits temporal variation in the introduction of the parking policy. Standard errors are clustered at the week-year level.

2.4 Results

In this section we first demonstrate that the policy had a large, robust, impact on both on-street and off-street parking demand, including the number of arrivals. We then investigate the impact on overall traffic flow and examine the heterogeneity within the day.

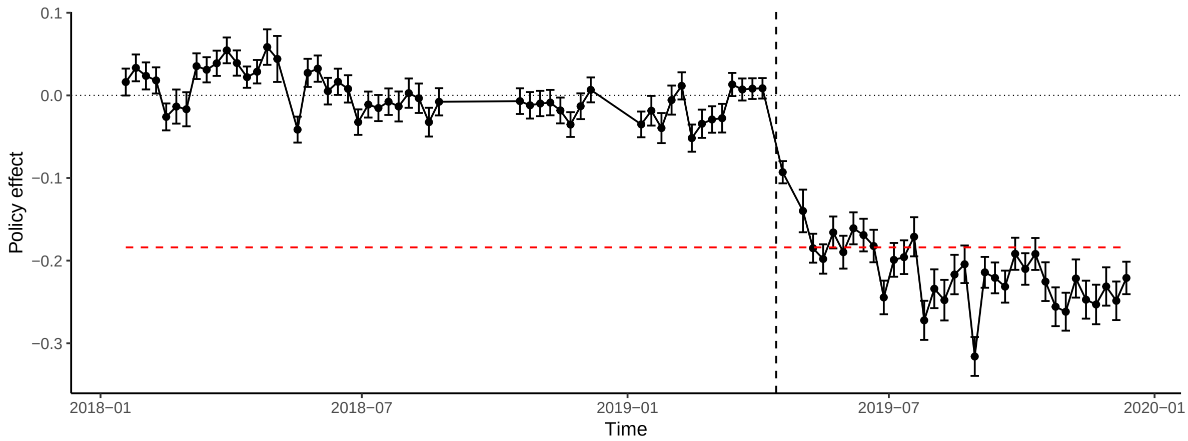


Figure 2.5: On-street parking volume policy effect after including all controls.

2.4.1 Parking

2.4.1.1 Identical trends

In Section 4.2 we have shown that on-street parking demand has a slight positive trend and a sharp decline after the policy is introduced. In Figure 2.5 we plot estimates of a weekly policy effect, while including all controls and fixed effects as in our preferred specification in (2.1).[35] Here, the coefficients are estimated by including year-week dummies and excluding the week prior to the introduction of the policy.[36] There appears to be no significant pre-trends, and the overall impact of the price increase in April 2019 is clear from the immediate and sustained drop in overall parking demand of around 20%. We find effects of around 10% for arrivals and duration (see Figures 2.B.1 and 2.B.2 in Appendix A).

[35]For the raw weekly aggregates see Figure 2.B.1 in Appendix B.

[36]Specifically, the figure plots the β_τ coefficients from estimating:

$$E[D_{it}] = \exp\left(\sum_{\tau=51}^{156} \beta_\tau P_{t-\tau} + \phi_i + \kappa \mathbf{S}_t + \tau \mathbf{W}_t + L(t)\right), \tag{2.5}$$

where $P_{t-\tau}$ is a year-week dummy and β_τ is the effect of the policy for each year-week t. Given week fixed effects in this setting, we omit the year-week dummies for 2017 and the missing weeks from 2018; otherwise perfect multicollinearity emerges. The error bars represent the 95% confidence interval for each weekly point estimate, clustered at the parking area level.

Table 2.2: Citywide results: On-street parking volume

	Volume				
	(1)	(2)	(3)	(4)	(5)
Policy effect	-0.178***	-0.182***	-0.187***	-0.184***	
	(0.007)	(0.007)	(0.007)	(0.007)	
Price citywide (log)					-0.366***
					(0.014)
Year 2019	0.019***	0.029***	0.026***	-0.032***	-0.032***
	(0.007)	(0.006)	(0.006)	(0.007)	(0.007)
Post week 15	-0.041***	-0.039***			
	(0.004)	(0.003)			
Time trend				0.040***	0.040***
				(0.005)	(0.005)
Area FE		Yes	Yes	Yes	Yes
Weekday FE			Yes	Yes	Yes
Week FE			Yes	Yes	Yes
Public holiday FE			Yes	Yes	Yes
School holiday FE			Yes	Yes	Yes
Observations	2,710,535	2,710,535	2,710,535	2,710,535	2,710,535
Pseudo R^2	0.0057	0.69107	0.71469	0.71491	0.71491

Notes: Estimated using Quasi-ML Poisson regression. Standard errors in parentheses are clustered at the parking area level. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

2.4.1.2 On-street parking

Table 2.2 shows the estimation results for parking volume with incremental levels of controls and fixed effects. Column (1) shows that with only a year dummy and a post week 15 dummy, we find a statistically significant effect of 16.3%.[37] Columns (2) and (3) show that controlling for parking area fixed effects and time varying controls (day-of-week, week-of-year, public and school holiday fixed effects, and weather controls) has essentially no effect on the coefficient of interest. In column (4) we replace the year dummy with an annualised daily linear time trend. The coefficient on the time trend indicates that there is a positive, and statistically significant, increase in parking

[37] The year dummy captures annual growth in parking demand over time and the post week 15 dummy captures seasonal differences in demand over the year which are correlated to the introduction of the policy, such as summer school holidays. Note, the coefficients from a Poisson model can be interpreted as a percentage change using $(e^{\beta} - 1) \cdot 100\%$.

Table 2.3: Citywide results: On-street arrivals and duration

	Arrivals			Duration		
	(1)	(2)	(3)	(4)	(5)	(6)
Policy effect	-0.091***	-0.096***		-0.090***	-0.091***	
	(0.005)	(0.005)		(0.004)	(0.003)	
Price citywide (log)			-0.191***			-0.181***
			(0.010)			(0.007)
Year 2019	0.016***	-0.036***	-0.036***	-0.0007	0.0002	0.0002
	(0.006)	(0.006)	(0.006)	(0.003)	(0.004)	(0.004)
Post week 15	-0.061***			0.025***		
	(0.003)			(0.002)		
Time trend		0.038***	0.038***		0.0010	0.0010
		(0.005)	(0.005)		(0.003)	(0.003)
Area FE		Yes	Yes		Yes	Yes
Weekday FE		Yes	Yes		Yes	Yes
Week FE		Yes	Yes		Yes	Yes
Public holiday FE		Yes	Yes		Yes	Yes
School holiday FE		Yes	Yes		Yes	Yes
Observations	2,710,535	2,710,535	2,710,535	2,655,737	2,655,737	2,655,737
Pseudo R^2	0.00258	0.68113	0.68113	-23.52087	-21.08404	-21.08404

Notes: Estimated using Quasi-ML Poisson regression. Duration is weighted by the average number of arrivals per parking area. Standard errors in parentheses are clustered at the parking area level. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

demand by around 4% annually.[38]

Our preferred specification in column (4), which controls for parking area fixed effects, seasonality, and trends, implies that the citywide effect of the parking policy resulted in 16.8% fewer on-street parking hours. In column (5) we replace the post indicator with mean citywide on-street parking prices pre and post policy. The result indicates that the citywide parking demand elasticity with respect to parking prices is equal to -0.37.

In Table 2.3 we estimate the policy and price effect on arrivals and duration. Both effects remain highly stable to the introduction of controls. In our preferred specification (column (2)), the number of arrivals declines by 9.2%, which corresponds to a citywide price elasticity of -0.19, while average duration declines by 8.7% with a city-

[38]We divide the daily time trend by 365 so the coefficient can be interpreted as an annual effect.

Table 2.4: Results: offstreet parking.

	On-street	Off-street	Volume Combined (on & off)	P&R	Combined (all)
	(1)	(2)	(3)	(4)	(5)
Policy effect	-0.190***	-0.058***	-0.173***	0.049**	-0.148***
	(0.003)	(0.014)	(0.003)	(0.019)	(0.003)
Area FE	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes
Public holiday FE	Yes	Yes	Yes	Yes	Yes
School holiday FE	Yes	Yes	Yes	Yes	Yes
Observations	1,434,729	6,514	1,441,243	2,797	1,444,040
Pseudo R^2	0.73038	0.72621	0.82366	0.47118	0.87003

Notes: Subsample of on-street parking data starting on 2018-07-04. Garages in column (3) and (5) are weighted by the inverse proportion of garage capacity in the sample as compared to the entire off-street parking market. In effect, commercial garages get a weight of 3.2 and P&R facilities get a weight of 1.6 each. Standard errors in parentheses are clustered at the year-week level. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

wide price elasticity of -0.18.[39] It appears that the citywide price elasticity of arrivals is approximately equal to the price elasticity of duration (by construction, the sum is approximately equal to the volume elasticity).[40]

2.4.1.3 Off-street parking and overall parking demand

Our estimates for the impact of the parking policy on on-street parking ignores the effect on the off-street parking market. The policy may have increased off-street demand as drivers substitute away to off-street parking or it may have decreased off-street demand because garage prices increased. In Table 2.4 we estimate the policy impact on parking volume at commercial off-street garages.

One issue is that we have a much shorter period of observation for off-street parking demand (only from July 2018). To examine the importance of having a shorter ob-

[39] Note the effect on duration captures that drivers park for a shorter duration and driver sorting, i.e. that drivers with long durations stopped parking.

[40] Arrivals increase by about 4% annually, while duration remained constant, consistent with trends in Figure 2.3.

served period, in column (1) of Table 2.4 we first re-estimate the main results in the on-street market for parking volume over the same time period for which we have off-street data. Due to the shorter time period and detailed set of temporal controls, the year dummy and time trend (included in Table 2.2) cannot be identified, however the effect of interest is essentially identical to our main result. This implies that controlling for this variable is not essential and this specification can be used to estimate the effect on off-street parking.

In column (2) we estimate the impact on commercial off-street garage demand and find a negative and statistically significant effect of around 5%. In column (3) we estimate the effect on on-street and off-street demand combined. The estimate indicates that the combined parking demand declined by 15.9% due to the policy. In column (4) we find a positive and statistically significant effect of around 5% on P&R facilities. This makes sense as P&R prices did not change and therefore these garages became more attractive. As P&R arrivals are small compared to on-street parking arrivals (2.35% arrivals), these findings have little impact on overall traffic flow within the city.[41] In column (5) we estimate the policy effect on the demand for on-street, off-street, and P&R combined. The estimated effect, which represents the overall citywide impact of the parking policy on the entire hourly parking market is 14%.[42] This implies that the policy did not result in a net increase in demand for off-street parking, but even a decrease because off-street garages responded by raising prices, albeit to a lesser degree than on-street.

We do not have information on off-street parking arrivals, but our estimates for volume suggest that the policy effect on on-street arrivals is likely an underestimate of the total policy effect on traffic flow. Under the assumption that the reduction in off-street arrivals accounts for half of the reduction in off-street parking volume, as is the case with on-street parking (see Table 2.3), these results indicate that off-street arrivals declined by about 2.5%, or about a quarter of the percentage decline in on-street parking (and about one eighth in absolute value). As this estimate is based on an assumption which seems plausible, but we cannot test, later on we will also make the more conservative assumption that the reduction in on-street volume is entirely due to a reduction in duration, so there was no net effect on the on-street arrivals.

[41] Applying the estimate from column (4) suggests that overall arrivals increased by around 48 cars due to P&R, or 0.61% of the reduction in total daily on-street parking. In addition, P&R garages are all outside the main city limits, so for traffic flow *within* the city, it is plausible that this effect is negligible.

[42] In Table 2.B.7 of Appendix B, we further examine the sensitivity of off-street parking demand to other specifications. We show that the effects are similar when we focus only on parking garages located in the city centre, control for changes in short-term capacity throughout the week, and include an extended period until February 2020.

Table 2.5: Local on-street parking demand elasticities

	Volume		Arrivals		Duration	
	(1)	(2)	(3)	(4)	(5)	(6)
Price (log)	-0.429***	-0.431***	-0.208***	-0.195***	-0.208***	-0.205***
	(0.019)	(0.035)	(0.015)	(0.028)	(0.008)	(0.014)
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes		Yes		Yes	
Week FE	Yes		Yes		Yes	
Public holiday FE	Yes		Yes		Yes	
School holiday FE	Yes		Yes		Yes	
Date FE		Yes		Yes		Yes
Observations	2,710,535	2,710,535	2,710,535	2,710,535	2,655,737	2,655,737
Pseudo R^2	0.71649	0.7175	0.68148	0.68242	-21.07682	-21.06902

Notes: Estimated using Quasi-ML Poisson regression. Duration is weighted by the average number of arrivals in a parking area. Standard errors in parentheses are clustered at the parking area level. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

2.4.1.4 Temporal and spatial variation

On-street parking price increases varied throughout the city between 0% and 100% (see Figure 2.A.4 in Appendix A). We therefore expect drivers to react to spatial differences in prices within the city to varying degrees, and areas where price increases are higher should face larger reductions in demand as compared to areas where price increases are lower.

In columns (1), (3), and (5) of Table 2.5, we estimate equation (2.2).[43] The results indicate that the average *local* price elasticities are somewhat higher than the citywide effects in Table 2.3, and are equal to -0.43, -0.21, and -0.21, for on-street volume, arrivals, and duration, respectively. In columns (2), (4), and (6) we estimate equation (2.3), therefore time-varying controls are essentially absorbed by the day fixed effects and the regression exploits spatio-temporal variation in the difference in changes in parking prices between areas. The elasticities are of a similar magnitude and still statistically significant at the 1% level, despite larger standard errors.

These local estimates may be biased downwards (overestimate) if prices in neighbouring districts are cheaper as this may cause drivers to substitute over space. There-

[43] In these regressions, we exploit both temporal *and* spatial variation in on-street parking prices, so we can interpret the price coefficients as on-street parking elasticities at the local level.

fore, in Table 2.B.1 in Appendix B we show that controlling for differences in on-street parking prices within 500 m and excluding areas within 500 m of a price boundary has little effect on the results, suggesting that substitution over short distances is relatively minor. This is not surprising as prices decline gradually with distance to the city centre, also after the increase in prices. Therefore changes in price discontinuities over space are small.

2.4.1.5 Robustness checks

Our preferred specification of the citywide effect indicates that on-street parking arrivals decline by around 9% due to the policy. Furthermore, we find that it is unlikely that many drivers substitute to off-street parking and we find similar policy effects locally. In this section we perform a range of additional robustness checks. Tables and additional discussion of the results are available in Appendix B.

Our key identification assumption relies on the correct specification of the time trend. Therefore, in Table 2.B.2 we consider how the specification of the time trend impacts the estimated policy effect on arrivals. Some areas may have become more attractive for parking over time, so we interact the time trend with parking price regimes, but find essentially identical effects. It may also be the case that time trends are non-linear, so we allow for a flexible time trend by adding a third-order polynomial term and find that the policy effect becomes somewhat smaller.[44] To abstract from long-run trends we also estimate the policy effect using a shorter time window around the introduction in week 16, 2019. We gradually make the time interval larger from one month pre-post to two and then three months pre-post. The results suggest that the short-run effects are similar in magnitude to the estimated policy effect with the linear time trend, which may indicate that the non-linear time trend is absorbing part of the policy effect of interest. Therefore, in our main estimates and in further analysis, we apply a linear time trend.

In the main analysis we exclude the Northern part of Amsterdam because they experienced an expansion in the parking area in July, 2018. In Table 2.B.3, we include the Northern part of Amsterdam and a specific time trend for new areas and find essentially identical results. Furthermore, prices are the highest in the city centre and fall with distance to the periphery. Therefore, we also estimate the policy effect separately for central and non-central parking zones and find that the policy effect and

[44] The second-order term on the time trend is negative and significant, which is not intuitive, as there is no convincing explanation for why parking demand should fall in a time of strong economic growth. This suggests that there is 'overfitting', which makes this specification less convincing.

price elasticity of arrivals in central zones is around 50% larger than the effect outside these areas.

Motorists may be substituting to other on-street options. This is relevant because other parking policies may have (un)intentional consequences. In Table 2.B.4 we examine to what extent drivers substituted to discounted €0.10 shopping areas (with time limits of either one or three hours) and visitor permits as a result of the policy. We show that there was a significant increase in arrivals of around 7% at inner city shopping areas (which have one hour time restrictions) while there was no increase in demand in the peripheral industrial parking areas (which have three hour time limits). This result is interesting as shopping areas generate substantially more traffic per parking space as they have a higher turnover.[45] Finally, we show that visitor permit demand increased substantially as a result of the policy by around 65%, which indicates that residents make significantly more use of these discounts due to the policy, although it is still a small share of total arrivals (1.8% after the policy).

Finally, standard errors may be too small if parking demand is serially positively correlated (Bertrand et al., 2004). To address this issue, we cluster our standard errors at the time-invariant level of a parking area. In addition, we run a robustness check where we focus only on time-series variation around the policy introduction and aggregate our data into six periods, pre and post week 15 in each year (therefore, for each parking area, we have only six observations). Table 2.B.6 presents the results. The results are essentially identical to our main estimates and the standard errors only slightly increase.

2.4.2 Traffic

2.4.2.1 Implied traffic effects using parking estimates

Our main estimate indicates that on-street arrivals decline by around 9%, whereas off-street arrivals decrease by around 2.5%. Given 84,600 daily on-street arrivals pre-policy, the arrivals effect implies there are around 7,800 fewer cars travelling within the city due to the policy. Furthermore, given the off-street estimate of 2.5% and around 42,000 daily off-street arrivals, this implies an additional reduction of up to 1,200 arrivals, or an overall reduction in flow of around 9,000 cars. Travel surveys indicate that there are approximately 640,000 daily (one-way) car trips within the paid

[45] It follows that the policy effect would have been much larger in the absence of these discounted shopping areas, and would be much smaller in the hypothetical case that Amsterdam would have much more discounted shopping areas.

Table 2.6: Main results: Traffic flow.

	Flow (log)				
	(1)	(2)	(3)	(4)	(5)
Policy effect	-0.030***	-0.021***	-0.021***	-0.028***	
	(0.007)	(0.006)	(0.006)	(0.005)	
Price citywide (log)					-0.055***
					(0.011)
Post week 15	0.005				
	(0.006)				
Year 2019	0.036***	0.030***			
	(0.006)	(0.006)			
Time trend			0.031***		
			(0.006)		
Loop-direction FE	Yes	Yes	Yes	Yes	Yes
Weekday FE		Yes	Yes	Yes	Yes
Week FE		Yes	Yes	Yes	Yes
Public holiday FE		Yes	Yes	Yes	Yes
School holiday FE		Yes	Yes	Yes	Yes
Observations	12,696	12,696	12,696	12,696	12,696
R^2	0.93704	0.96235	0.96235	0.96937	0.96937

Notes: Standard errors in parentheses are clustered at the week-loop level. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

parking area of Amsterdam, excluding tourists. Under the assumption that trips that end in on-street parking travel a similar distance within the city, this implies around 2.4% – 2.8% less traffic flow as a result of the policy.[46]

2.4.2.2 Traffic effects based on loop data

In Table 2.6 we directly examine the effect on traffic flow by estimating equation (2.4). In column (1) we include a year dummy, a post week 15 dummy, and loop-direction fixed effects, which are important because we have an unbalanced panel and traffic flow varies by location. We find that the policy results in around 3.0% less traffic flow. In columns (2) and (3), when we control for additional time-varying controls and a linear trend, the effect becomes smaller and equal to around 2.1%. Finally, in

[46]For every return trip, we assume one parking action. Survey data on trips within Amsterdam support this assumption with few trips including more than one destination. Therefore, $\frac{9{,}000}{0.5\times 640{,}000} = 2.8\%$, of which 2.4% corresponds to reductions in on-street parking.

column (4) we interact the time trend with each loop-direction fixed effect to capture area specific trends and find that the policy reduces traffic by around 2.8%.

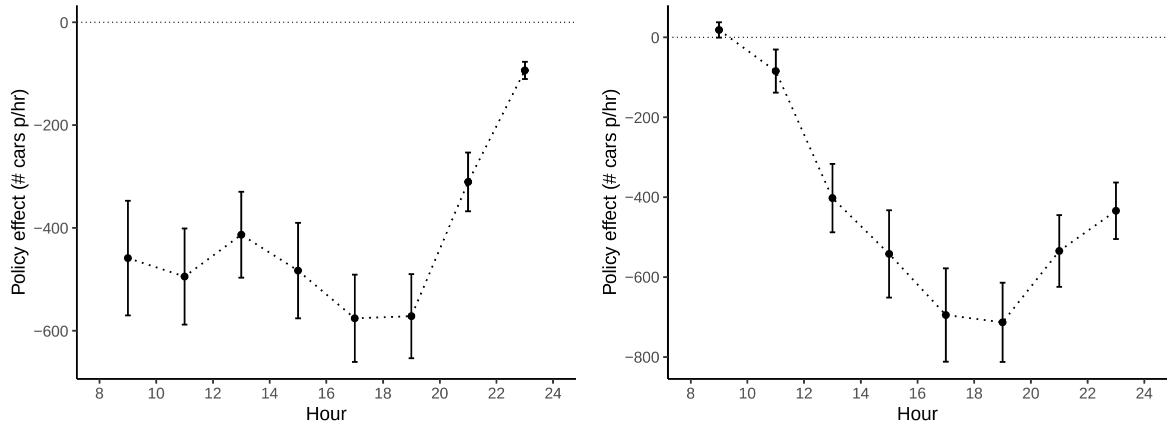
Consequently, this implies that the citywide effect of the parking policy results in around 2% – 3% less traffic flow. The estimated effect is in line with the estimates implied by the impact of the parking policy on parking demand. In column (5) we replace the policy dummy with citywide on-street parking prices pre and post policy. This implies that the citywide traffic elasticity with respect to prices, is around -0.06, or a quarter of the size of the arrivals elasticity in Table 2.3, consistent with the share of (hourly) paid on-street parking in the total number of daily trips.

2.4.2.3 Implied traffic effects within the day using parking estimates

Up until now, we have focused on the effect of the policy on arrivals and traffic flow at the daily level. However, when used as a second-best congestion policy, it is more efficient when the policy reduces traffic during peak hours of the day. Generally, it is believed that hourly parking charges only reduce congestion by reducing the total number of trips, as fees do not differentiate by how much a given driver adds to congestion (Small and Verhoef, 2007). Therefore, in this section we examine how the policy effects vary *within the day*. We emphasise here that we also focus on exits, as within the day, the effects on arrivals and exits differ from each other.

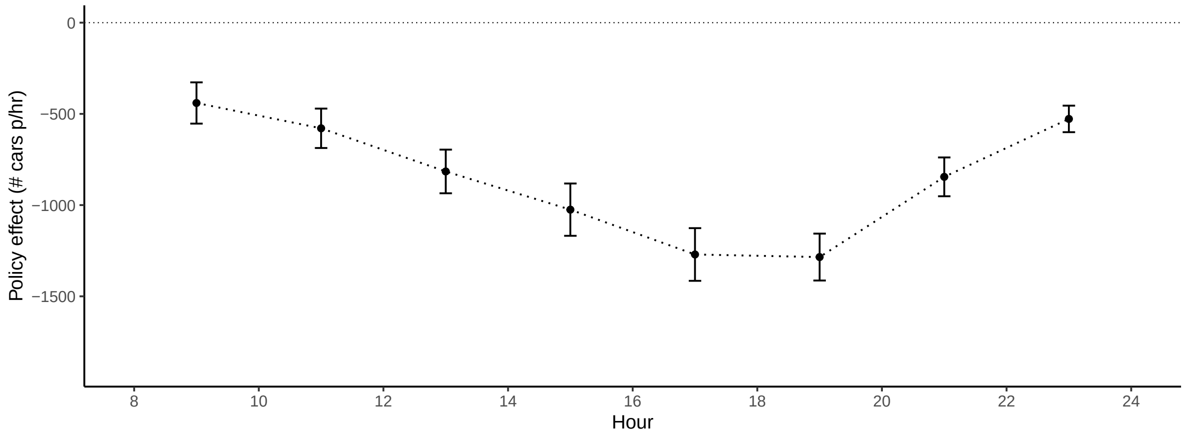
Figure 2.6 plots the effect of the policy on the number of cars arriving or exiting within the day. Here we estimate the level effect because it is the absolute number of cars during peak hours that matters for congestion. Panel (a) indicates that the policy effect on arrivals is relatively uniform up to 20:00 and becomes becomes smaller late in the evening as there are few arrivals during this time. Panel (b) shows that the policy effect on exits is largest in the evening peak hours between 16:00 – 20:00. In panel (c), we provide an estimate of the citywide reduction in traffic flow generated by on-street parking within the day. We find that the largest reductions in traffic are in the afternoon peak hours between 16:00 – 20:00 (a reduction of around 1,300 cars), which is more than double the reduction between 08:00 – 12:00 (around 500 cars).

This traffic effect is driven by two key factors. First, the traffic generated by on-street parking varies within the day, with the sum of arrivals and exits peaking between 14:00 – 16:00 (see Figure 2.A.7 in Appendix A). Second, the behavioural responses to prices differ within the day, with larger arrival and exit price elasticities in the evening (see Figure 2.B.6 in Appendix B). This is in line with trip purpose data from travel diaries which indicate that most activities involving parking are not work related (see Figure 2.A.10 in Appendix A).



(a) Arrivals

(b) Exits



(c) Total effect on daily traffic

Figure 2.6: Policy effect based on parking data within the day.

2.4.2.4 Traffic effects within the day using loop data

In Figure 2.7 we show the policy effects and the implied total effect on citywide traffic within the day. This is calculated by multiplying the estimated hourly effects in percentages by the mean number of trips for each hourly interval (see Figure 2.B.7 in Appendix B).[47] Although the standard errors are larger, we find a similar patten and similar order of magnitude to the on-street parking estimates from Figure 2.6.

[47]The mean number of trips per hour are estimated based on the proportion of traffic over the day (from the loop data) and the total number of car trips (from travel survey data), where the number of trips T for each hour h equals: $T_h = \frac{MeanFlow_h}{\sum_{i=8}^{24} MeanFlow_i} \times T_{day}$.

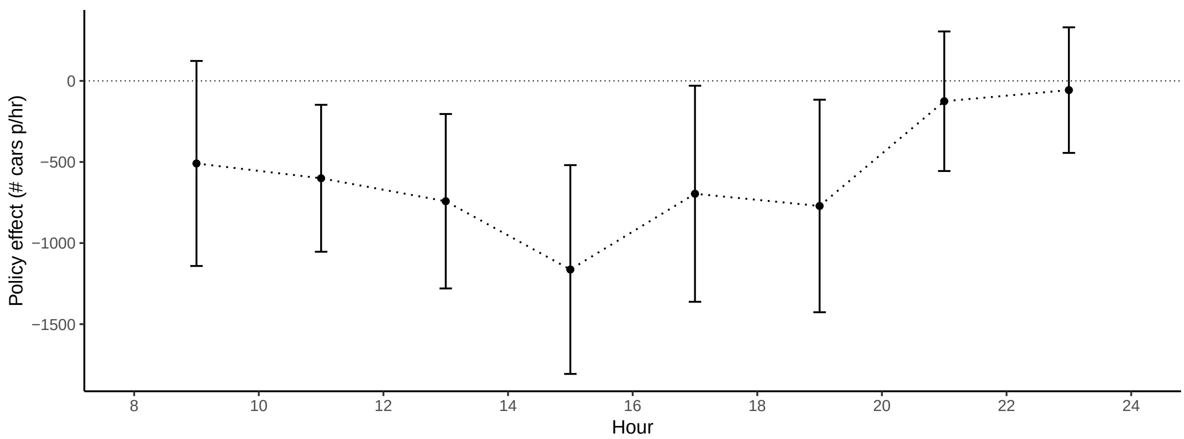


Figure 2.7: Policy effect based on traffic flow data within the day.

2.4.3 Counterfactuals

2.4.3.1 Welfare implications

A higher on-street parking price may generate societal benefits, because it reduces cruising and travel externalities from congestion, pollution, and accidents, while also freeing up parking space for other uses. Furthermore, it generates revenues that can be used to finance public goods, such as parks and pedestrian walkways. It also increases the producer surplus of commercial parking operators. Meanwhile, it will also impose societal costs in the form of a lower consumer surplus due to higher on-street and off-street prices. In this section we aim to provide a back-of-the-envelope welfare calculation where we distinguish between the implications to residents, non-residents, and commercial operators (see Section 7 in Appendix B for details).

In order to get a benchmark estimate we make several simplifying assumptions. First, we assume that, conditional on the supply of off-street parking and the provision of residential permits, on-street parking prices *after the policy* are at the socially optimal level. This is potentially a restrictive assumption, but it seems plausible in the light that hourly on-street parking prices were lower than hourly off-street prices *and* on-street prices had remained constant for the last 10 years, despite strong increases in prices of substitutes (housing, commercial land, and off-street parking), as well as increases in national income and car ownership. Second, we assume that prices for commercial providers are above marginal costs. This makes sense because there is no free entry into the off-street market in Amsterdam and therefore off-street parking supply is essentially fixed.

We first calculate the daily welfare effects, excluding travel externalities. Daily parking demand is approximately equal to 213,000 hours on-street and 106,000 hours off-street before the policy. Considering that the policy caused parking demand to decline by 17% on-street (36,000 hours) and 5% off-street (5,000 hours), the net gain to society is approximately €27,000 per day.[48]

This benefit however excludes travel externalities in the form of congestion, pollution, and accidents. Our estimates suggest that the policy caused the number of car trips to decline by about 2.4% (15,600 trips). Taking into account that the average trip distance *within* Amsterdam is around 7 km, this implies that overall VKT in the city declined by about 109,000 km.[49] The passenger vehicle externality of petrol cars (the sum of congestion, pollution, and accident externality) is thought to be about €0.12 per km in the Netherlands (Schroten et al., 2014), slightly above the (implicit) marginal tax on fuel of €0.09 per km, therefore the societal benefits are approximately €3,000. This may be an underestimate because pollution externalities are larger in urban areas and we exclude VKT outside of Amsterdam. Nevertheless, pollution only accounts for a small share of marginal external costs and motorists may substitute trips to other locations, so these effects may be small.

The above benefit also excludes cruising for parking. Arnott et al. (2015) find that in a static parking market where on-street and off-street parking are perfect substitutes, the number of cars cruising for parking is proportional to on-street arrivals and the fee differential between on-street and off-street parking. This differential was reduced by €0.40 per hour. Given an average parking duration of 2.4 hours, this implies that the policy reduced the willingness to pay to avoid on-street parking search by up to €0.96. Taking the average VOT for car travel in the Netherlands of around €15.40, this roughly translates to travel time savings of around 4 minutes. Given 77,000 daily arrivals after the policy, the expected increase in welfare is maximally €74,000 per day. This may be a large overestimate because (a) cruising only occurs at peak hours, (b) we ignore price differences between discounted shopping areas, and (c) motorists may prefer to park off-street for reasons other than avoiding private cruising costs, and (d) some garages are cheaper for day parking. Therefore we assume that the private cruising gains are around one quarter of the size (€18,000 per day), but acknowledge that this estimate has extreme uncertainty.

[48]This is equivalent to the rule of half ($0.5 \times dP \times dQ$). In the on-street market, supply can be replaced by other uses, therefore the policy leads to welfare gains, whereas in the off-street market, parking becomes idle which is a welfare loss (maximum capacity off-street during the day is generally below 80%). Hence reductions in demand on-street lead to a welfare gain (of €30,000) but changes off-street results in a small welfare loss (of €3,000).

[49]This is potentially a gross underestimate of the total distance reduction as non-residents travel much longer distances, on average 36 km, outside of Amsterdam.

Adding up the gains in the parking market (€27,000), the reduction in traffic externalities (€3,000), and the gains from less cruising (€18,000) implies an overall daily societal gain of around €48,000 due to the price increase. This gain however masks substantial heterogeneity between residents, non-residents, and commercial operators. The total daily gains to residents are approximately €195,000, commercial profits increase by around €52,000 (of which one third, €17,000, goes overseas), and non-residents lose around €196,000 (see Appendix B for calculations). Given that there are around 850,000 inhabitants in Amsterdam, this suggests that the *annual* gains are around €84 per inhabitant. These benefits are largely in the form of increased government revenues (35%) and the hypothetical value of reclaimed land previously designated to on-street parking.

2.4.3.2 Automated vehicles

In the near future, automated vehicles (AVs) will not require parking close to their destination. This has implications for parking demand in cities because AVs will either not park at all or will be able to park outside the city where parking is cheaper (Gelauff et al., 2019; Millard-Ball, 2019). We make several (heroic) assumptions on how AVs might operate and apply our estimates to gauge the order of magnitude impacts of AVs on traffic flow in the city centre and in the periphery of Amsterdam in a partial equilibrium setting (see Section 8 in Appendix B for more details).

We first consider a (private ownership) AV scenario where all motorists, currently using (hourly) paid on-street parking, park outside the city and pay cheaper rates. Given that the proportion of traffic generated by on-street parking is around one quarter, our estimates for the price elasticity of arrivals and duration imply that traffic flow is expected to increase by about 27% – 33%, of which 2 and 8 percentage points are generated by new car trips due to the lower parking prices in the periphery and in the city centre, respectively, but the majority (25 percentage points) is generated by empty AVs travelling to and from parking facilities outside the city.

In the alternative (shared) AV scenario, AVs do not drive to the periphery but parking prices become essentially zero. Our estimates then imply that duration increases by 2.9 hours in the city centre and 2.6 hours in the periphery, while traffic increases by around 16% and 12%, respectively.

This counterfactual application assumes that there will be *no policy intervention*. This is unlikely as parking is heavily regulated in most cities and AVs are likely to have large effects on traffic and government revenues, so local governments may respond by implementing road pricing or other vehicle restrictions.

2.5 Conclusion

In this paper, we provide novel evidence on the effect of parking policy on citywide parking demand and traffic flow. We use temporal variation from a large citywide increase in average hourly on-street parking prices of 66%. Our findings show that overall on-street parking demand fell by around 17%, while the combined demand for the entire hourly parking market (on-street and off-street) declined by 14%. We do not find that the reduction in on-street parking is offset by an increase in demand off-street.

Our results also show that on-street parking arrivals declined by 9% which corresponds to a citywide parking price elasticity of -0.19. Taking into account that about one quarter of car trips in Amsterdam uses hourly on-street parking, this implies an effect on citywide traffic flow of around 2.4%. This result is confirmed using traffic road loop data, where we find a subsequent reduction in traffic flows around the city of around 2% – 3%, and larger effects in the evening. A back-of-the-envelope calculation suggests an increase in welfare, mainly enjoyed by local residents.

Our findings also have implications for policies that aim to reduce citywide traffic. Generally, it is believed that parking charges only reduce congestion by reducing the total number of trips, as fees do not differentiate by how much a given driver adds to congestion. Our results show that the parking policy had larger effects during evening peak hours because at these times, parking demand is more elastic and there is more traffic generated by on-street arrivals and exits. Theoretical models can better reflect reality by accounting for this heterogeneity.

Our study also has implications for policies aiming to replace on-street parking spaces with other uses. Even before COVID-19 forced many cafes, bars, and restaurants to spread out onto side-walks and parking spaces, cities around the world have been looking for new ways to improve the urban environment. Higher parking prices can be used as a policy tool to raise government revenues and convert on-street parking to other uses, such as parks, cycling lanes, and restaurants, without causing additional externalities from cruising or building new off-street capacity.

Further research should aim to study the long-term impacts of parking policy and examine the wider implications on modal choice and the decision to travel. It is unclear from this study whether drivers switch to other transport modes or stop travelling entirely. This has implications for the economic effects of transport policy, such as shop revenues and pedestrian activity.

Appendix 2.A Additional descriptives

2.A.1 Detailed policy context

Parking policy in Amsterdam is also described in detail in a range of studies such as Van Ommeren et al. (2011) and De Vos and Van Ommeren (2018). The total on-street parking supply in Amsterdam is 260,000 (including residential areas in the periphery), 163,000 of these are located in paid parking areas. Almost all on-street parking is shared between residents using permits and all other paid (hourly) parking. Minor exceptions to shared use include disabled parking spots, reserved for residents, and discounted shopping streets which have time limits of one hour and can not be used with a residential permit.

During the day, the majority of on-street parking demand comes from residents using cheap residential permits, with the remaining share available for hourly parking, which is substantially more expensive. For example, in the city centre, permit fees cost around €1.50 per day (€535 per year), while an identical on-street spot currently costs non-residents €80 per day. According to the transport department at the municipality, approximately 80% of on-street parking is occupied by residential permit holders during the day however, this is likely to vary by location in the city.

Around one-third of visitors parking in Amsterdam use off-street commercial parking garages, which are mainly provided by private operators and charge approximately the same price as nearby on-street parking. Cruising for parking is limited compared to other large cities because of high on-street prices however, relatively higher (pre-policy) off-street prices in the inner city may indicate the presence of cruising. Furthermore, cruising is likely to occur in the evening as residents come home from work and in a limited number of discounted shopping streets and industrial zones (€0.10), where one and three-hour parking duration restrictions apply, respectively.

Visitor permits allow residents to obtain a 50% – 75% discount on the hourly on-street fares to their visitors. The permit is limited to a maximum number of hours per residential household and can only be used within the close vicinity of the residence (valid within the residential parking zone). After the policy change, this limit increased from 10 or 30 hours to 40 hours per month.

Parkers always have to provide their license plate number when a transaction is initiated, tying the car identifier to a specific parking location. Vehicles from an enforcement entity, fitted with cameras, sweep the number plates of all vehicles parking on-street and send the information to a centralised system. Licence plates are then cross-checked against a database of paid (hourly) transactions and residential per-

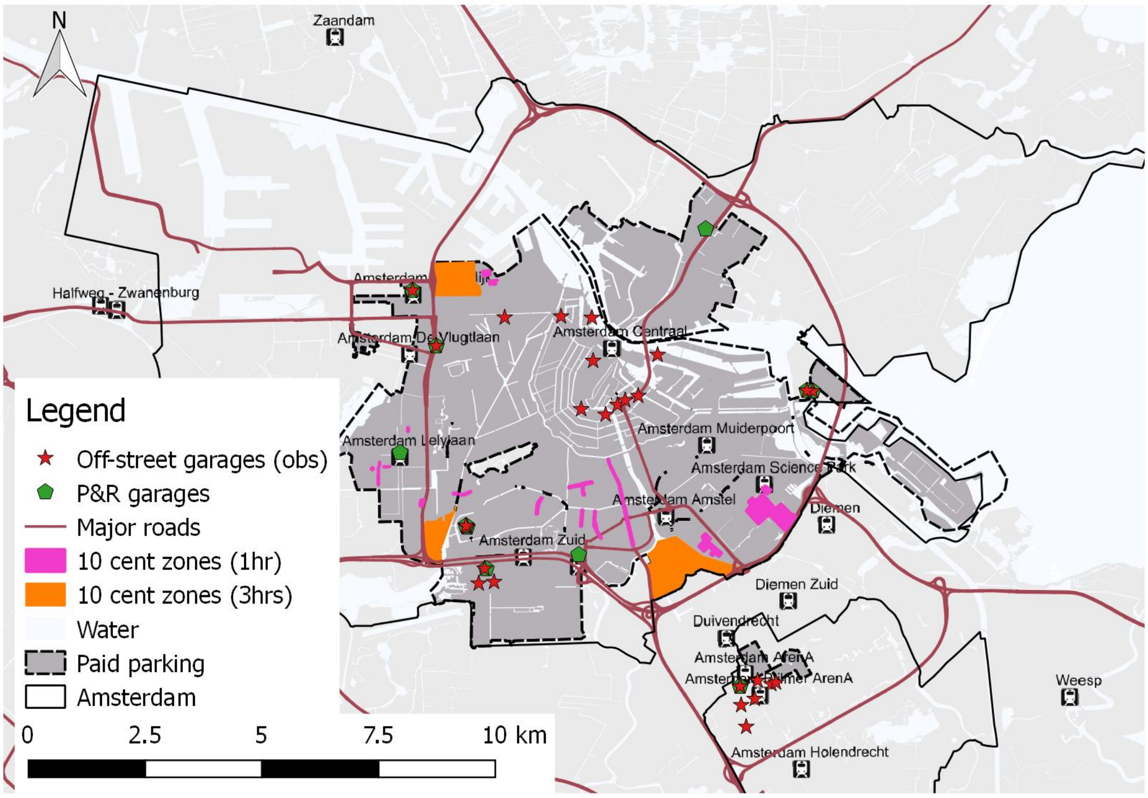


Figure 2.A.1: Parking infrastructure in Amsterdam (detailed).

mits. Local authorities are then automatically alerted when an infraction is detected and fines are sent to the address of the car owner (Egis Group, 2019). Parking fines increased by €15 in May 2019 from €47.60 to €62.60 in 2019. In 2017, there were 780,000 parking fines in the city, which represents about 2% of the annual number of parking arrivals and adds up to around €40 million in fines (Parool, 2019).

2.A.2 Substitutes to paid on-street parking

As discussed in the main text, the key issue is that motorists may switch to off-street parking. In Amsterdam, motorists parking without a residential permit may also switch to other on-street parking alternatives, namely discounted shopping zones (€0.10) designated for shopping and discounted visitor permits available to residents. In our analysis, we explicitly control for these factors by including these substitutes in our main regressions. Here we discuss these types of parking in more detail and the implications for our results.

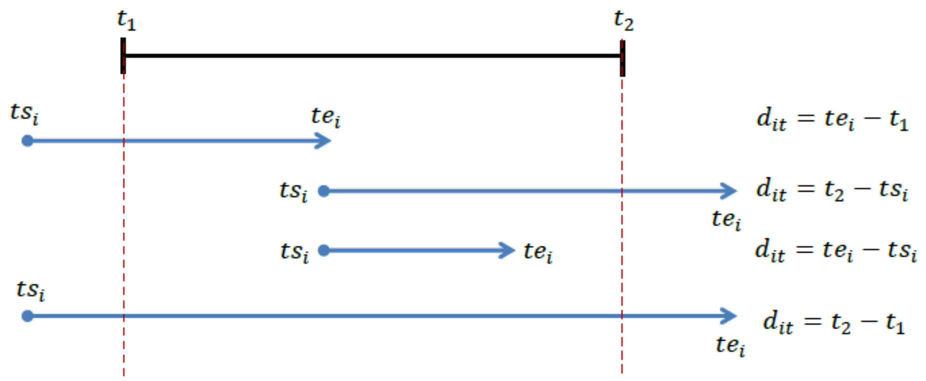


Figure 2.A.2: Aggregating transactions data to parking demand.

Figure 2.A.1 illustrates the areas with time restrictions and a fixed rate of €0.10, designated as shopping and industrial activities. Although these areas are small, the proportion of arrivals is not negligible (13.5% of all arrivals). Parking in these areas is particularly attractive for drivers parking up to one hour (26.88% of arrivals pre policy). So, we include these areas when estimating equation (2.1) and in a sensitivity check, examine to what extent separately estimating the effect on shopping areas affects our main results.

Residents have access to visitor permits that offer a 50% – 75% discount on the hourly paid on-street rates. This option becomes more attractive after the price increase and some car drivers that used to pay hourly rates may start using these visitor permits, so we also include parking demand at the residential parking zone level when estimating equation (2.1) and examine to what extent excluding these groups affects our main results.

2.A.3 Detailed data description

2.A.3.1 Aggregating parking data

In this section we describe in more detail how we aggregate the transaction data to create the parking demand variables in our analysis. We define:

- t_1 and t_2: as the interval start and end time,
- ts_i and te_i: as the transaction i start and end time.

Figure 2.A.2 illustrates the four distinct cases that a transaction can fall into and the approach to aggregate the data into daily (or hourly) data. The time window is represented by t_1 and t_2, which is the start and end of the day in our main application, so $t_1 = $ 00:00 and $t_2 = $ 11:59. In the first case from the top, the transaction starts *before* and ends *within* the time interval. In this case, the car is parked for $d_{it} = te_i - t_1$ hours during the time interval and corresponds to one exit. In the second case from the top, the transaction starts *within* and ends *after* the time interval. In this case, the car is parked for $d_{it} = t_2 - ts_i$ hours and corresponds to one arrival. In the third case from the top, the transaction starts *within* and ends *within* the time interval. In this case, the car is parked for $d_{it} = te_i - ts_i$ hours and corresponds to one arrival and one exit. In the last case, the transaction starts *before* and ends *after* the time interval. In this case, the car is parked for $d_{it} = t_2 - t_1$ hours and does *not* correspond to an arrival or an exit.

Therefore, we can calculate the total number of hours parked (volume), arrivals, and the mean duration per day (or any other time interval) and area as:

- Volume (parking hours): $V_t = \sum_{i=1}^{N} d_{it}$,
- Arrivals (number of cars): $A_t = \sum_{i=1}^{N} [t_1 \leq ts_i < t_2]$,
- Mean duration (of arrivals): $D_t = (\sum_{i=1}^{N} [t_1 \leq ts_i < t_2] D_i)/A_t$.

2.A.3.2 Holidays definition

In the Netherlands, there are five school holiday periods, of which two (Christmas and May) are the same for the entire country and three (spring holiday in February-March, summer holiday in July-Aug, and autumn holiday in October) are staggered by region and therefore start and end at different times. There are three school regions (north, middle, and south), of which Amsterdam is part of the Northern region, so we include school holidays from this region. In our analysis, we include separate fixed effects for each type of public and school holiday, and distinguish between whether the holiday falls on a weekday or weekend (in essence, we interact a weekend dummy with each holiday). Therefore we add a total of eight dummies for school holidays (week or weekend) and seven dummies for public holidays.

2.A.3.3 Off-street parking data

The majority of municipality-owned garages are located around the Bijlmer arena to the South-East of the central part of the city. Commercial and public garages located in the close vicinity charge similar prices.

P&R tickets are valid for the entire day, under the condition that drivers can show a validated travel card indicating that they used public transport to travel to and from the city centre on the day. There are three tariff types for P&R facilities. Peak hours (8.89% arrivals) which cost €10 per day, off-peak hours (63.18% arrivals) and weekend (27.94% arrivals) which cost €1 per day.

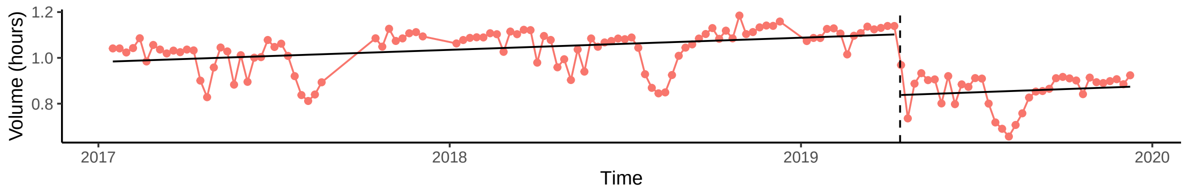
2.A.4 Trends

Figure 2.A.3 indicates that this relation holds for (hourly) paid parking and discounted shopping areas (which account for 98.51% of arrivals), while parking with a visitor permit essentially has no growth in demand up until the policy announcement at the end of October 2018, after which the trend appears to become positive. This is likely to be associated to the information relating to the upcoming policy adjustment and news articles about the availability of permits. Data from the municipality also indicates a substantial growth in the request for these permits around this time.

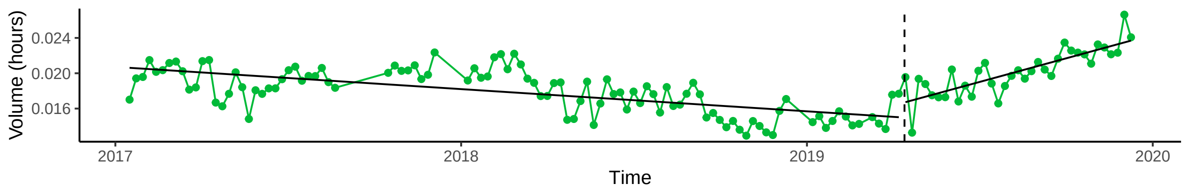
2.A.5 Distribution of key variables

2.A.5.1 On-street parking prices

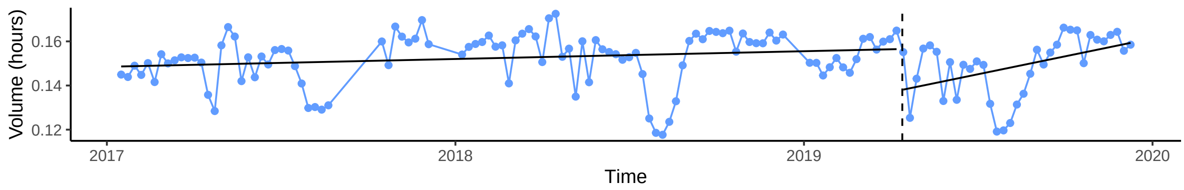
Figure 2.A.4 illustrates the effect of the parking policy on the distribution of parking prices, weighted by the number of arrivals. As can be seen, the policy results in a large shift in prices at all levels, except for discounted parking areas which remain unchanged. Panels (a) and (b) further illustrate the large absolute and relative change in parking prices. Panels (c) and (d) illustrate the absolute and relative change in prices throughout the city. It indicates that price changes varied substantially over space, but not uniformly with distance from the city centre. This is primarily because the delineation of some parking zones changed as a result of the policy.



(a) Paid (regular) parking



(b) Visitor permit



(c) Shopping areas

Figure 2.A.3: Sum of weekly on-street parking volume (millions of hours) by type.

2.A.5.2 On-street parking demand

Figure 2.A.6 illustrates the distribution of the dependent variables over the day. Volume is low before 06:00 and steadily increases throughout the day with the highest occupancies around 14:00, after which volume begins to decline. Parking arrivals are relatively uniformly distributed over the day with a spike at 09:00 when paid parking becomes active. There are also some arrivals before 09:00 because people using their phone or arriving early can already arrange parking to avoid forgetting and receiving a fine. Arrivals start to decline around 19:00. Exits rates are highest around 15:00, and also face a spike around 19:00 when many areas become free. Figure 2.A.7 illustrates the arrivals, exits, and the total per two hour interval. This essentially shows the distribution of traffic over the day generated by (hourly) on-street parking. The key take away is that traffic generated by on-street parking is relatively uniform between 10:00 – 18:00, with the peak between 14:00 – 16:00.

2.A.5.3 Off-street parking demand

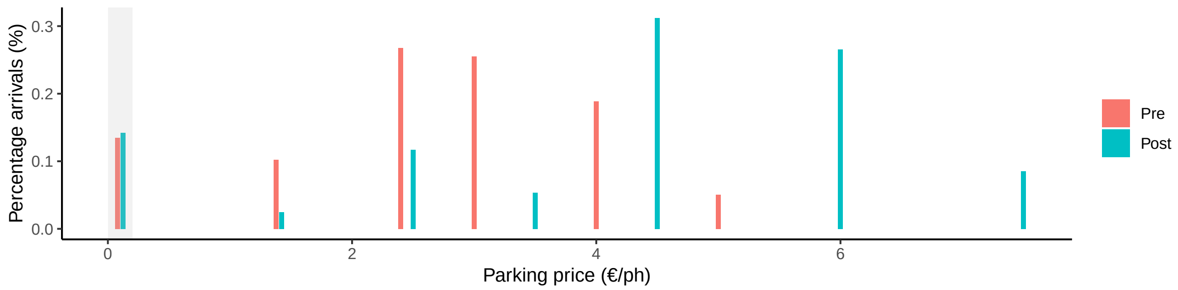
Figure 2.A.8 compares daily on-street and off-street parking volume (excluding P&R) per hour. We assume that the occupancy rate in our sample is representative for all garages and approximate off-street parking volume for all garages by multiplying the mean daily parking volume in our sample of garages by the proportion of total capacity ($1/0.31$). The figure illustrates that the proportion of off-street to on-street parking demand is approximately one third (0.36).

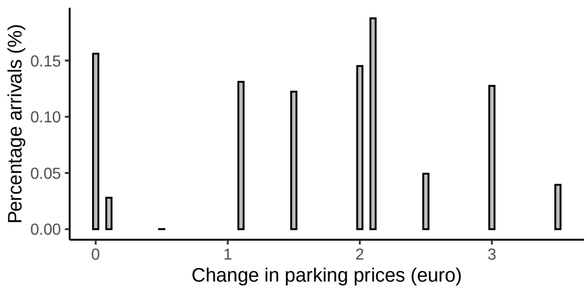
2.A.5.4 Traffic flow

Figure 2.A.9 illustrates the traffic flow and speed data. As can be seen, traffic flow is low in the evening hours and relatively uniform over the day, with a peak at 08:00 and 17:00. Traffic speed, as measured by the loop data, is also relatively uniform with only a small drop in average speed during the day when traffic flow is higher.

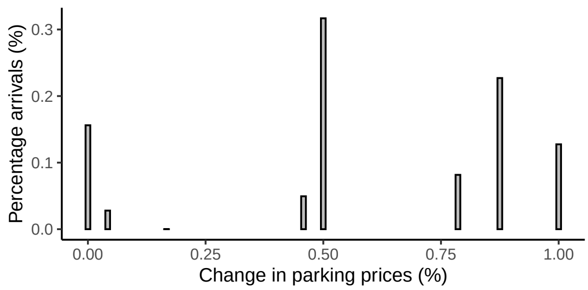
2.A.6 Additional descriptives

Figure 2.A.10 illustrates the stated purpose of car trips that end in the paid parking area of Amsterdam. We exclude trips by residents "returning home" as residents have access to permits and therefore these trips would generally not end in paid (hourly) parking. Overall, around one-third of trips are by commuters, which are more likely to be non-residents. These commuting trips may end in paid on-street parking however, commuters are likely to have access to (free) employer parking. The last three columns show that the proportion of non-work car trips is increasing throughout the day.

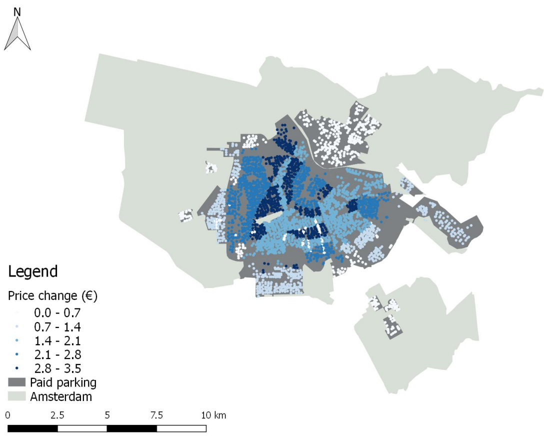




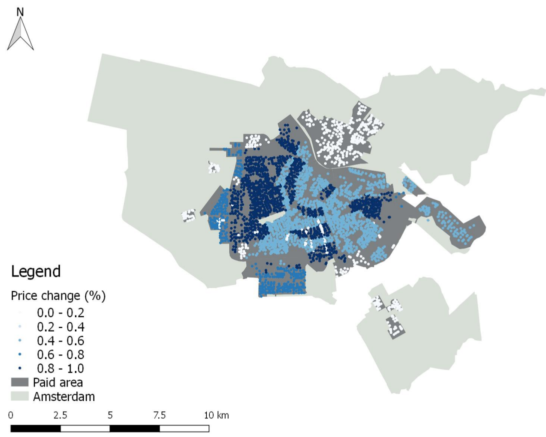
(a) Absolute change (€)



(b) Relative change (%)



(c) Absolute change (€)



(d) Relative change (%)

Figure 2.A.4: On-street parking prices pre and post policy.

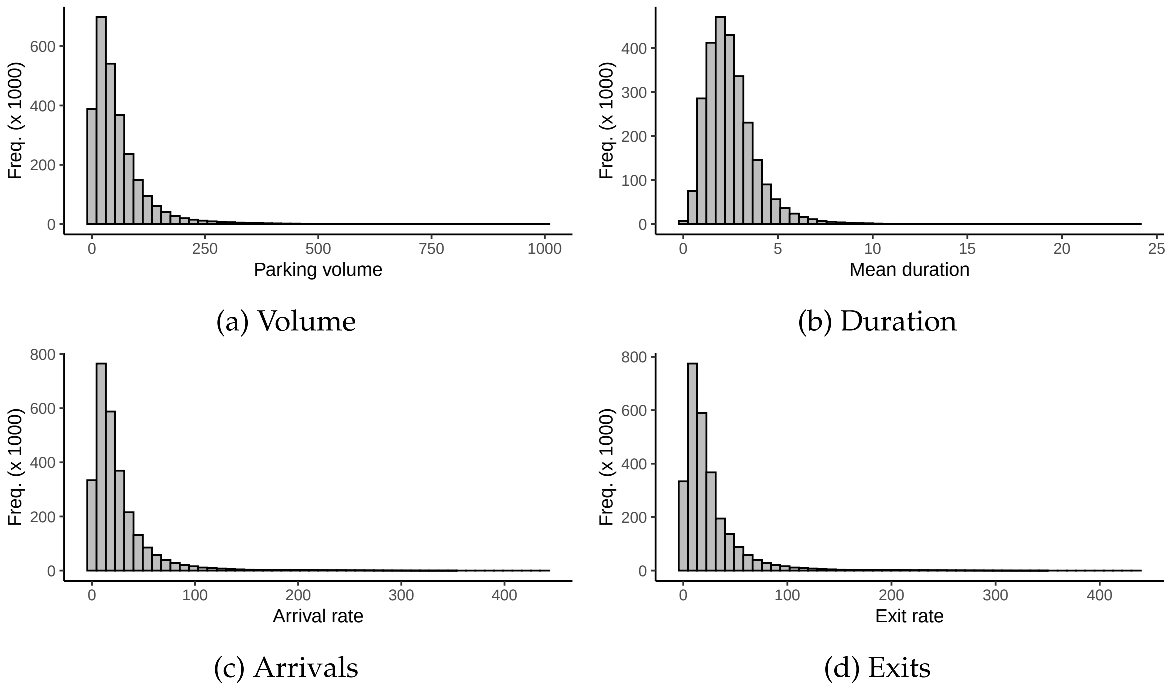


Figure 2.A.5: Histograms of on-street parking demand variables.

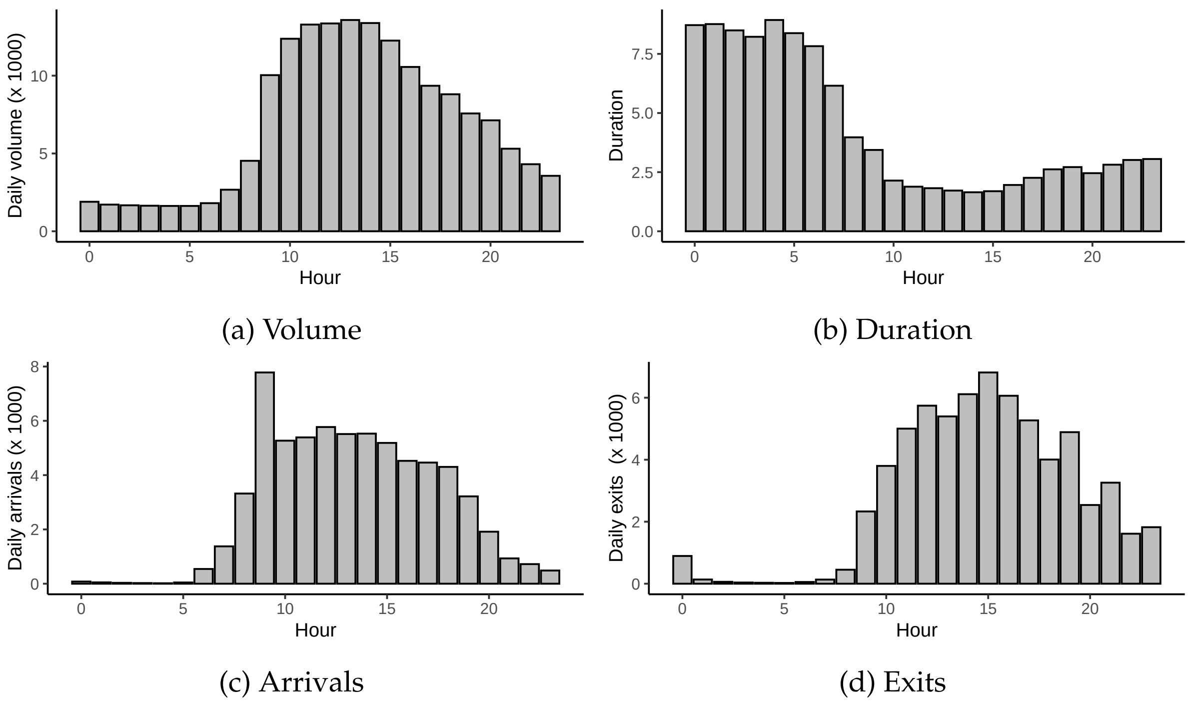


Figure 2.A.6: Histograms of hourly on-street parking demand.

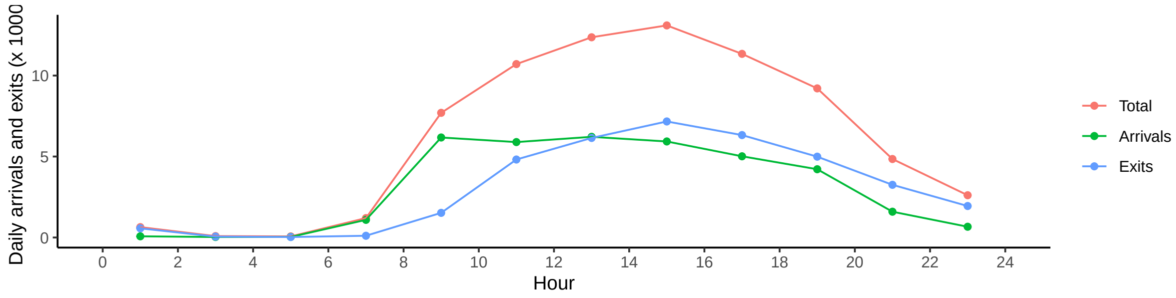


Figure 2.A.7: Traffic from on-street parking within the day.

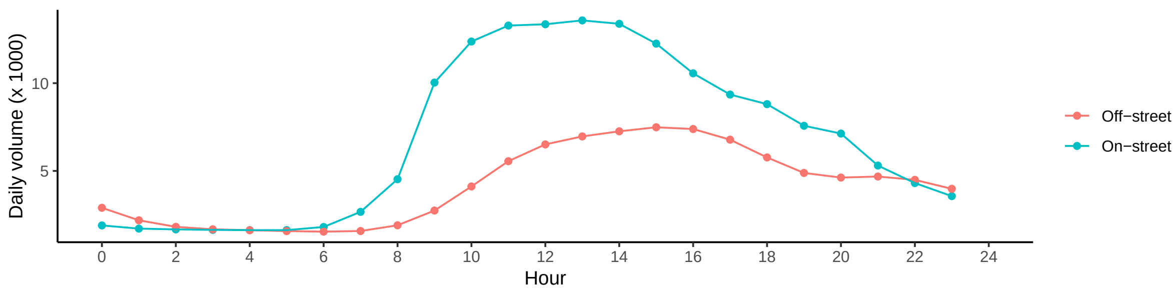
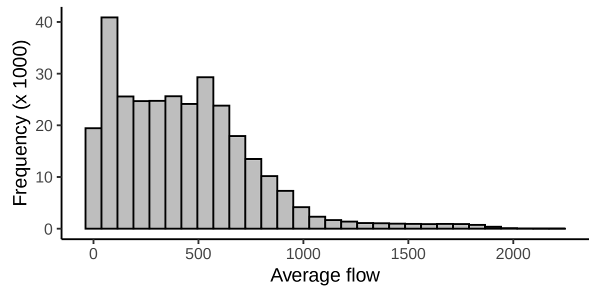
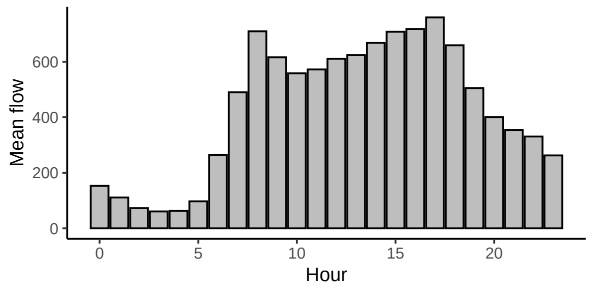


Figure 2.A.8: Parking volume on-street and off-street (estimated).



(a) Flow



(b) Flow per hour

Figure 2.A.9: Histograms of hourly traffic flow based on loop data.

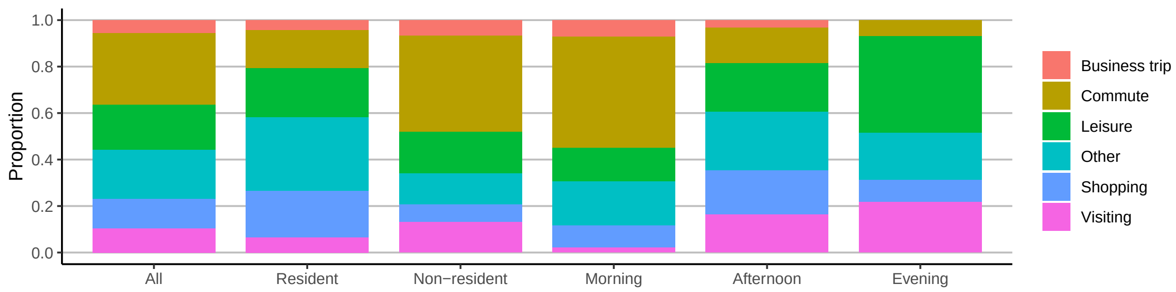


Figure 2.A.10: Purpose of car trips ending in Amsterdam excluding residential parking.

Appendix 2.B Additional results

2.B.1 Identical trends on-street parking

Our key identification is that in the absence of the policy, parking demand would have followed an identical trend as in the pre-period. In Figure 2.5 we show the estimated policy effect at the weekly level after including all controls. In this section, we show that the raw on-street parking trends show similar patterns to the estimated policy effect and that the effects for arrivals and duration are similar to the effect on parking volume. We also show the common trends for off-street parking demand and traffic flow.

Figure 2.B.1 shows that parking demand follows a very similar trend before and after the policy. Furthermore, the effect of the policy is clear from the sharp decline when the policy becomes active.

Figure 2.B.2 shows that the effect on arrivals is slightly more volatile than the effect on volume and duration, which show a sharper, more stable drop in demand. This is likely due to large events or activities around the city. While this may affect the precision of the policy estimate, it does not affect the consistency, under the assumption that the events are uncorrelated to the policy. This seems to be the case as the fluctuations appear to be random.

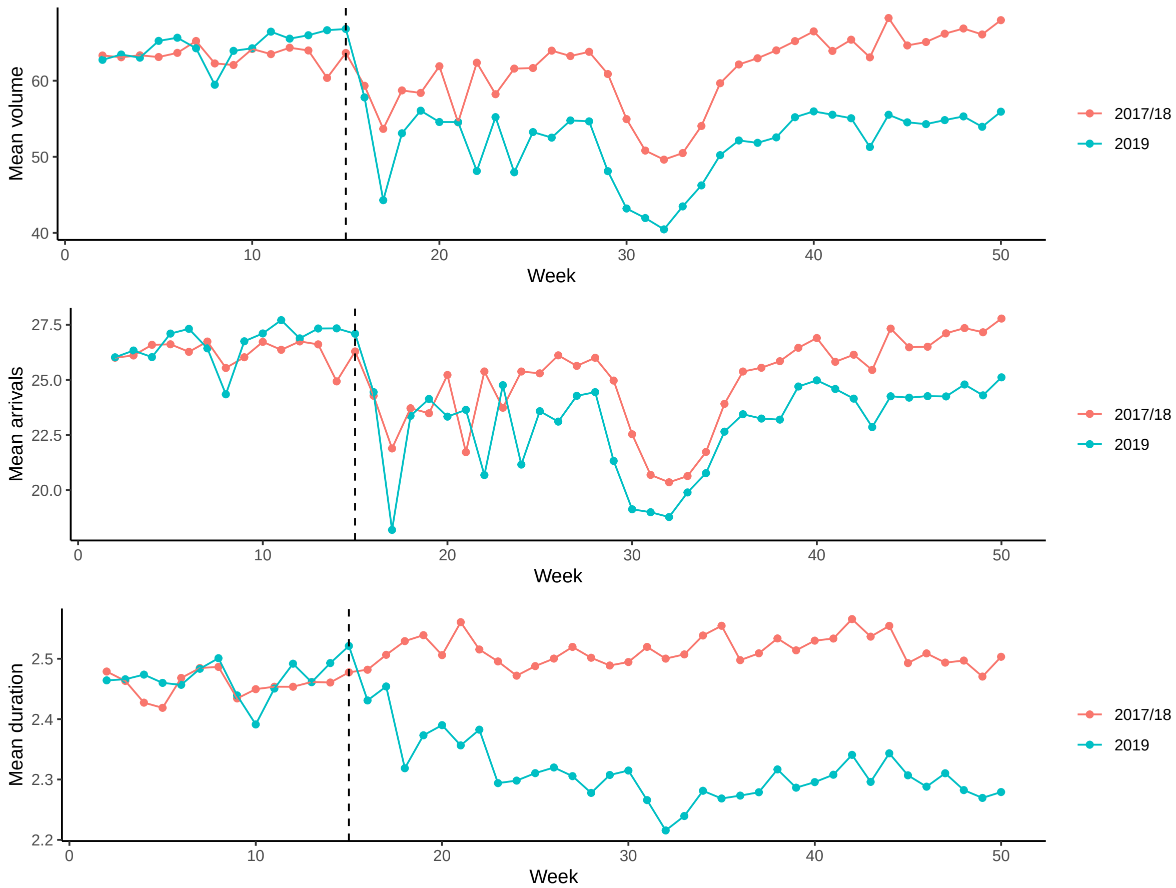
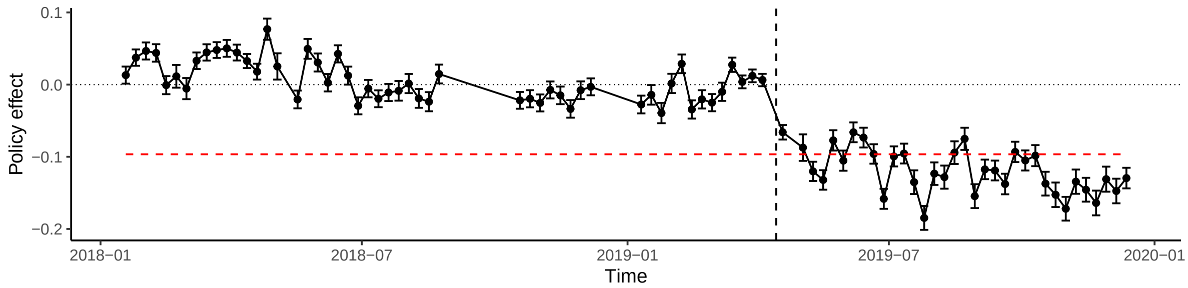
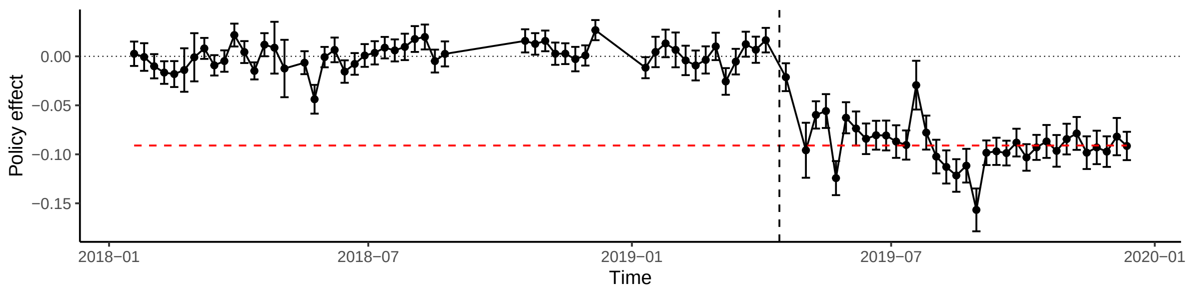


Figure 2.B.1: Mean on-street parking volume, arrivals, and duration per area.



(a) Arrival rate



(b) Duration

Figure 2.B.2: Estimated policy effect after including all controls.

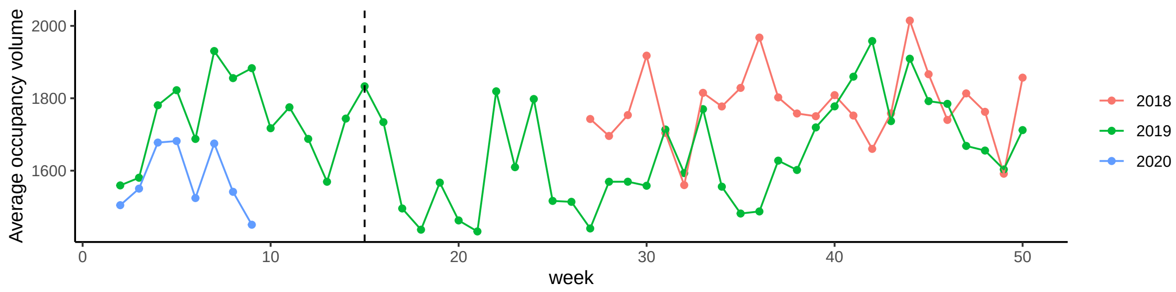


Figure 2.B.3: Commercial off-street parking identical trends.

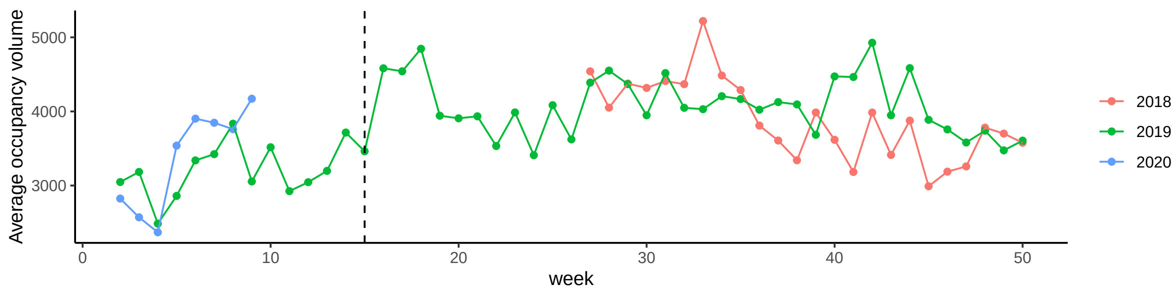


Figure 2.B.4: P&R off-street parking identical trends.

2.B.2 Identical trends off-street parking

Figure 2.B.3 and 2.B.4 show the raw trends in mean commercial off-street parking volume. The data is somewhat noisier because we have fewer observations as compared to the on-street data. However, there appears to be no discernible trend in commercial off-street parking, while demand is lower in the post period. Meanwhile, parking volume at P&R facilities appears to be declining over time, and there is a sharp increase when the parking policy is introduced. Demand then appears to shrink, and in early 2020, demand is similar to the same period in 2019. Note that due to the shorter time period for which we obtain off-street parking data, we cannot estimate the weekly policy effect after controls while including week fixed effects.

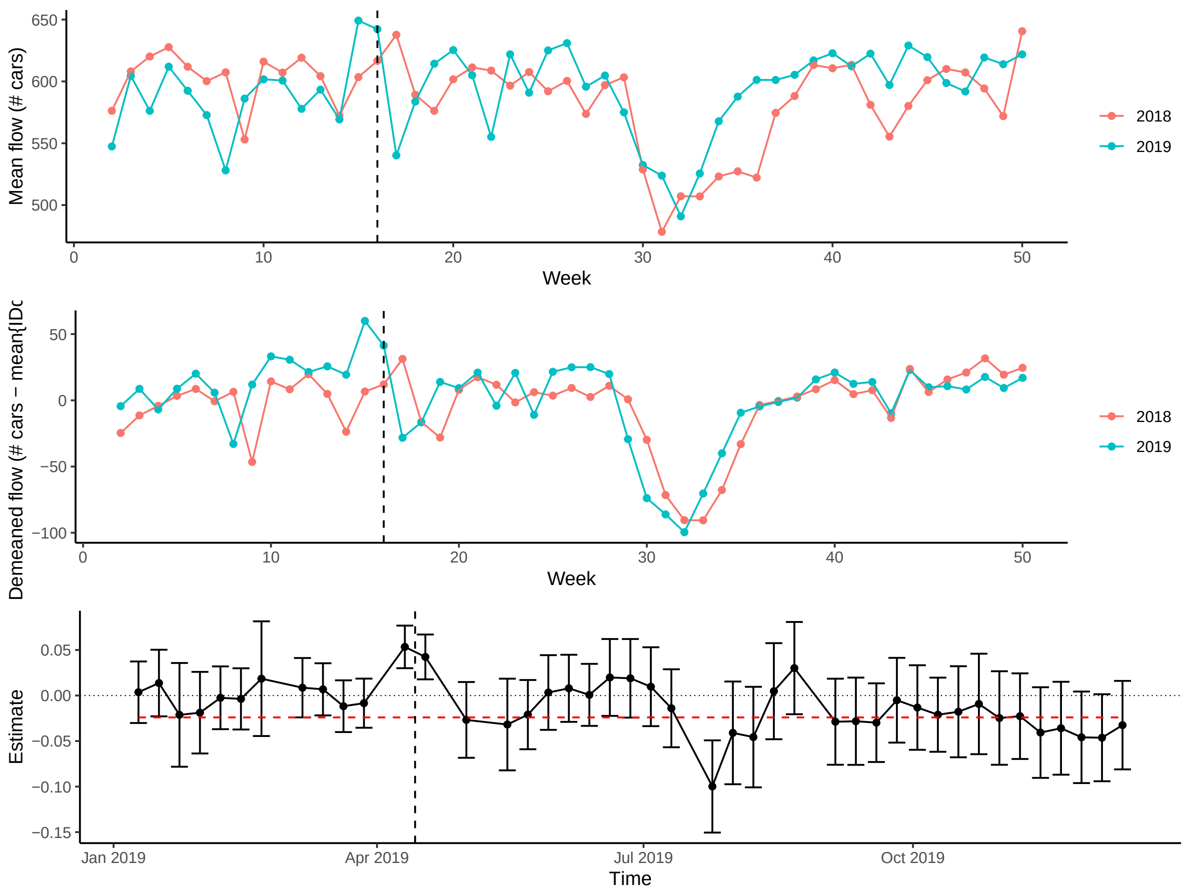


Figure 2.B.5: Aggregate traffic flow trends.

2.B.3 Identical trends traffic flow

Figure 2.B.5 presents the trends in traffic flow. The top plot indicates that flow appears to follow similar trends to on-street parking demand with a noticeable dip around the summer school holiday period. However, the aggregate data is quite noisy because the panel is unbalanced and road traffic is unequally distributed over the network so the mean is somewhat sensitive to missing data. Therefore in the middle plot, we demean the data by the average flow per traffic loop. The trend becomes clearer, although it is difficult to discern a significant policy effect. Therefore in the bottom plot, we control for all temporal and spatial factors as in Table 2.6 and find that the effect is approximately 2% however, we acknowledge that the effect is still quite noisy at the weekly level and is somewhat sensitive to summer vacations and other holidays.

Table 2.B.1: Sensitivity: spatio-temporal variation. Local results: On-street parking.

	Volume		Arrivals		Duration	
	Diff 500m	Excl 500m	Diff 500m	Excl 500m	Diff 500m	Excl 500m
	(1)	(2)	(3)	(4)	(5)	(6)
Price (log)	-0.434***	-0.377***	-0.217***	-0.224***	-0.218***	-0.190***
	(0.019)	(0.073)	(0.015)	(0.045)	(0.008)	(0.027)
Price difference (500 m)	-0.063***		-0.056***		-0.009	
	(0.019)		(0.016)		(0.007)	
Price difference^2 (500 m)	0.013		0.003		0.009*	
	(0.010)		(0.008)		(0.005)	
Price difference^3 (500 m)	0.007***		0.004***		0.003***	
	(0.002)		(0.001)		(0.0009)	
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Public holiday FE	Yes	Yes	Yes	Yes	Yes	Yes
School holiday FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,709,707	349,429	2,709,707	349,429	2,654,913	341,875
Pseudo R^2	0.71699	0.70869	0.68185	0.64658	-21.0789	-18.42167

Notes: Estimated using Quasi-ML Poisson regression. Standard errors in parentheses are clustered at the parking meter level. Season FE include day-of-week, week-of-year, and public holidays. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

2.B.4 Robustness on-street parking

2.B.4.1 Local price effects and neighbouring prices

The local estimates in Table 2.5 may be biased downwards (overestimate) if prices in neighbouring districts are lower, which may cause drivers to substitute over space. In Table 2.B.1 we show that controlling for differences in on-street parking prices within 500 m and excluding areas within 500 m of a price boundary has little effect on the results, suggesting that substitution over short distances is relatively minor. This is not surprising as prices decline gradually with distance to the city centre, also after the increase in prices. Therefore changes in price discontinuities over space are small.

2.B.4.2 Specification of time trend

In Table 2.B.2 we consider how time trends affect the results. In column (1) we interact the time trend with eight parking rate zones post policy, including €0.10 shopping areas, and add a separate category for residential permit zones. This captures potential linear differences in the attractiveness of parking areas over time, for example, because more tourists visit the city centre by car. The results suggest that the policy effect on arrivals is essentially the same and equal to 9.2%. In column (2) we include a flexible time trend by adding a third-order polynomial term and find that the arrivals rate effect declines to 6%. In column (3) we interact the flexible time trends with parking rate zones (as in column (1)) and find that the effect becomes 5.8%. In column (4) – (6) we examine the policy effects using a shorter time period event study approach in the spirit of regression discontinuity design. Therefore, we exclude week fixed effects and time trends. We gradually make the time interval larger from one-month pre-post to two and then three months pre-post. The results suggest that the short-run effects are around 10%, which is slightly larger than the estimated policy effect that takes longer-term trends into account.

2.B.4.3 Heterogeneity location

In the main analysis, we exclude the North part of Amsterdam because they experienced an expansion in the parking area in July 2018. In Table 2.B.3, we include parking data from the North part of Amsterdam and a specific time trend for new areas which captures growth in demand in these areas over time. We find essentially identical results with parking arrivals decline by about 8.9% as a result of the policy. Furthermore, prices are the highest in the city centre and fall with distance to the periphery. Therefore, we also estimate the policy effect separately for central and non-central parking areas and find that the arrival effect is about 11.5% in central areas, which is around 50% larger than the effect outside these areas (7.9%) and is also reflected in a larger price elasticity.

2.B.4.4 Heterogeneity discounted shopping and visitor permits

We have included parking observations in discounted €0.10 shopping areas (with time limits of either one or three hours) and visitor permits in all our main on-street analysis. It is likely that the policy positively affected demand for these parking options as they became relatively cheaper. In Table 2.B.4 we examine to what extent drivers substituted to these options as a result of the policy. First, we examine to

Table 2.B.2: Sensitivity: long term trends. Citywide results: On-street parking.

	Arrivals					
	Zone × trend	Flex trend	Zone × flex trend	1 month	2 months	3 months
	(1)	(2)	(3)	(4)	(5)	(6)
Policy effect	-0.097***	-0.062***	-0.060***	-0.104***	-0.117***	-0.113***
	(0.005)	(0.005)	(0.006)	(0.004)	(0.005)	(0.005)
Year 2019	-0.037***	-0.004	-0.003			
	(0.006)	(0.006)	(0.006)			
Time trend		0.104***				
		(0.016)				
Time trend^2		-0.039***				
		(0.012)				
Time trend^3		0.003				
		(0.002)				
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes			
Public holiday FE	Yes	Yes	Yes	Yes	Yes	Yes
School holiday FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,710,535	2,710,535	2,710,535	177,926	336,011	494,087
Pseudo R^2	0.68209	0.6812	0.68289	0.71678	0.70417	0.69892

Notes: Estimated using Quasi-ML Poisson regression. Standard errors in parentheses are clustered at the parking meter level. Season FE include day-of-week, week-of-year, and public holidays. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

what extent the estimates change if we exclude observations of these two options. The first three columns indicate that the policy effect on arrivals is slightly stronger when excluding demand for visitor permits, but is much stronger when excluding demand for discounted shopping areas. This makes sense as only 1.3% of arrivals use residential permits while a relatively large proportion (13.5%) use shopping areas.

Columns (4) and (5) indicate that this result is mainly driven by an increase in demand for parking in inner-city shopping areas (which have one hour time restrictions) where arrivals increased by around 7%, while there was no increase in demand in the peripheral industrial parking areas which offer three hour time limits. This result is interesting as shopping areas generate substantially more traffic per parking space as they have a higher turnover. It follows that the policy effect would have been much larger in the absence of these discounted shopping areas, and would be much smaller in the hypothetical case that Amsterdam would have much more discounted shopping areas. Column (6) indicates that parking using visitor permits increased

Table 2.B.3: Sensitivity: Area. Citywide results: On-street parking.

			Arrivals		
	Incl North	Centre	Centre	Non-centre	Non-centre
	(1)	(2)	(3)	(4)	(5)
Policy effect	-0.093***	-0.122***		-0.082***	
	(0.005)	(0.006)		(0.007)	
Price citywide (log)			-0.233***		-0.179***
			(0.012)		(0.015)
Year 2019	-0.034***	-0.029***	-0.029***	-0.040***	-0.040***
	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)
Time trend (daily/365)	0.042***	0.024***	0.024***	0.047***	0.047***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Time trend (daily/365) × IDnewNoord	0.278***				
	(0.028)				
Area FE	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes
Public holiday FE	Yes	Yes	Yes	Yes	Yes
School holiday FE	Yes	Yes	Yes	Yes	Yes
Observations	2,867,996	1,093,002	1,093,002	1,617,533	1,617,533
Pseudo R^2	0.69644	0.61186	0.61186	0.70532	0.70532

Notes: Estimated using Quasi-ML Poisson regression. Standard errors in parentheses are clustered at the parking meter level. Season FE include day-of-week, week-of-year, and public holidays. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

substantially as a result of the policy by around 65%.[50]

2.B.4.5 Sensitivity to spatial aggregation

In Table 2.B.5 we assess the sensitivity of our main results at various spatial scales and compare the estimates from a Poisson and log model. Columns (1) – (3) indicate that the policy effect on parking volume is identical when estimated using a Poisson model. Meanwhile, the results from a log model are largest when estimated at the level of the parking meter and become smaller when areas are aggregated to the visitor permit zone and then to the aggregated parking zones. Furthermore, the

[50]This effect is a combination of the increase in on-street parking prices (so the discount increased in absolute value), more information about the availability of discounts, and an increase in the number of available hours from 10 or 30 to 40 hours per household per month.

Table 2.B.4: Sensitivity: substitutes. Citywide results: On-street parking arrivals.

	Arrivals					
	Excl 10c	Excl Res	Excl both	Only 10c 1hr	Only 10c 3hr	Only Res
	(1)	(2)	(3)	(4)	(5)	(6)
Policy effect	-0.116***	-0.104***	-0.126***	0.063***	0.0003	0.502***
	(0.005)	(0.005)	(0.005)	(0.022)	(0.021)	(0.050)
Year 2019	-0.034***	-0.036***	-0.034***	-0.050	-0.044	-0.048
	(0.005)	(0.006)	(0.005)	(0.042)	(0.035)	(0.040)
Time trend (daily/365)	0.046***	0.039***	0.047***	-0.089***	0.096***	-0.035
	(0.004)	(0.005)	(0.004)	(0.027)	(0.026)	(0.024)
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Public holiday FE	Yes	Yes	Yes	Yes	Yes	Yes
School holiday FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,587,339	2,658,249	2,535,053	71,109	52,087	52,223
Pseudo R^2	0.64749	0.6789	0.64388	0.62668	0.83756	0.79011

Notes: Estimated using Quasi-ML Poisson regression. Standard errors in parentheses are clustered at the parking meter level. Season FE include day-of-week, week-of-year, and public holidays. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

estimates in the log model are most consistent with the Poisson model at a more aggregated level. We conclude from this exercise that the log model is more sensitive to the level of spatial aggregation and that the Poisson model is preferred as it does not suffer from aggregation issues and provides a conservative estimate of the policy effect.

2.B.4.6 Standard errors

Standard errors may be too small if parking demand is serially positively correlated (Bertrand et al., 2004). To address this issue, we cluster our standard errors at the time-invariant level of a parking area. In addition, we run a robustness check where we focus only on time-series variation around the policy introduction and aggregate our data into four periods, pre and post-policy in 2018 and 2019.[51] Table 2.B.6 presents the results. The results are essentially identical to our main estimates, and the standard errors only slightly increase. This rules out any autocorrelation in error terms

[51]Therefore, for each parking area ID we have four observations.

Table 2.B.5: Sensitivity: Spatial aggregation. On-street parking.

	Volume			Volume (log)		
	Meter	Visitor permit	Agg zone	Meter	Visitor permit	Agg zone
	(1)	(2)	(3)	(4)	(5)	(6)
Policy effect	-0.184***	-0.185***	-0.185***	-0.208***	-0.193***	-0.187***
	(0.007)	(0.016)	(0.031)	(0.008)	(0.015)	(0.028)
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Public holiday FE	Yes	Yes	Yes	Yes	Yes	Yes
School holiday FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,710,535	64,202	7,452	2,660,882	64,202	7,452
Pseudo R^2	0.71491	0.96301	0.99132	0.35161	0.74497	1.13052

Notes: Column (1) – (3) are estimated using Quasi-ML Poisson regression without weights. Column (4) – (6) is estimated using OLS and is weighted by the mean number of arrivals per parking area. Standard errors in parentheses are clustered at the parking area level.***, **, * indicate significance at 1%, 5%, and 10%.

Table 2.B.6: Sensitivity: Standard errors. On-street parking.

	Volume	Arrivals	Duration
	(1)	(2)	(3)
Policy effect	-0.175***	-0.093***	-0.090***
	(0.007)	(0.005)	(0.004)
Post week 15	-0.042***	-0.059***	0.024***
	(0.003)	(0.003)	(0.002)
Year 2019	0.028***	0.025***	-1.563×10^{-5}
	(0.007)	(0.006)	(0.003)
Area FE	Yes	Yes	Yes
Observations	19,707	19,707	19,707
Pseudo R^2	0.85468	0.76404	-21.96828

Notes: Estimated using Quasi-ML Poisson regression. Parking demand is aggregated into pre and post in 2018 and 2019, therefore we omit all time series variation other than the policy effect. Standard errors in parentheses are clustered at the parking area level.***, **, * indicate significance at 1%, 5%, and 10%.

and highlights that our results and standard errors are hardly affected by serial correlation.

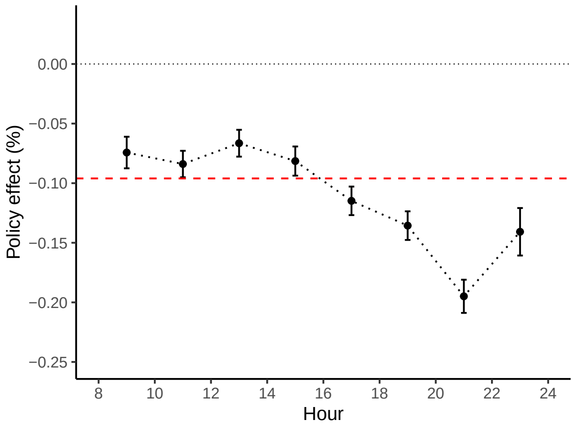
Table 2.B.7: Sensitivity: offstreet parking.

	Volume			
	City centre	Capacity	Incl. 2020 (Off)	Incl. 2020 (P&R)
	(1)	(2)	(3)	(4)
Policy effect	-0.073***	-0.070***	-0.069***	0.057***
	(0.016)	(0.014)	(0.012)	(0.021)
Capacity (dynamic)		-0.002***		
		(0.0003)		
Year 2019				-0.009
				(0.021)
Area FE	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
Public holiday FE	Yes	Yes	Yes	Yes
School holiday FE	Yes	Yes	Yes	Yes
Observations	5,713	6,514	7,269	3,126
Pseudo R^2	0.7563	0.73151	0.72393	0.44097

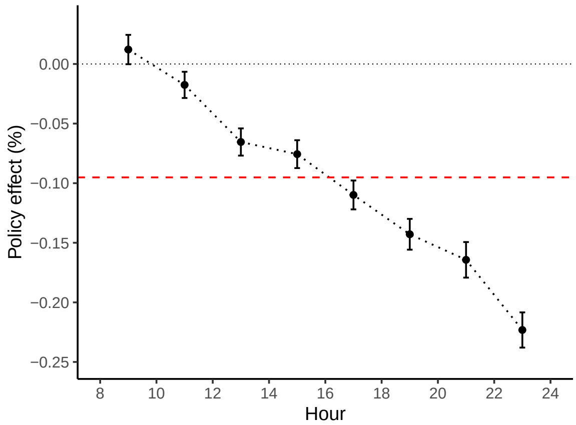
Notes: Estimated using Quasi-ML Poisson regression. Standard errors in parentheses are clustered at the year-week level. Season FE include day-of-week, week-of-year, and public holidays. Weather controls include the average daily temperature, windspeed, and a dummy for whether there was rain or temperatures below 0 °C between 8-20hr.***, **, * indicate significance at 1%, 5%, and 10%.

2.B.5 Robustness off-street parking

In Table 2.B.7 we further examine the sensitivity of off-street parking demand to other specifications. Column (1) shows that the effects are slightly larger when we exclude parking garages outside the city centre. In column (2), we also control for changes in short-term capacity throughout the week which may occur if commercial garages are also providing long-term parking to residents. The result also becomes slightly larger. In column (3) and (4) we include an extended period until February 2020, which also gives similar estimates to our main results.



(a) Arrivals

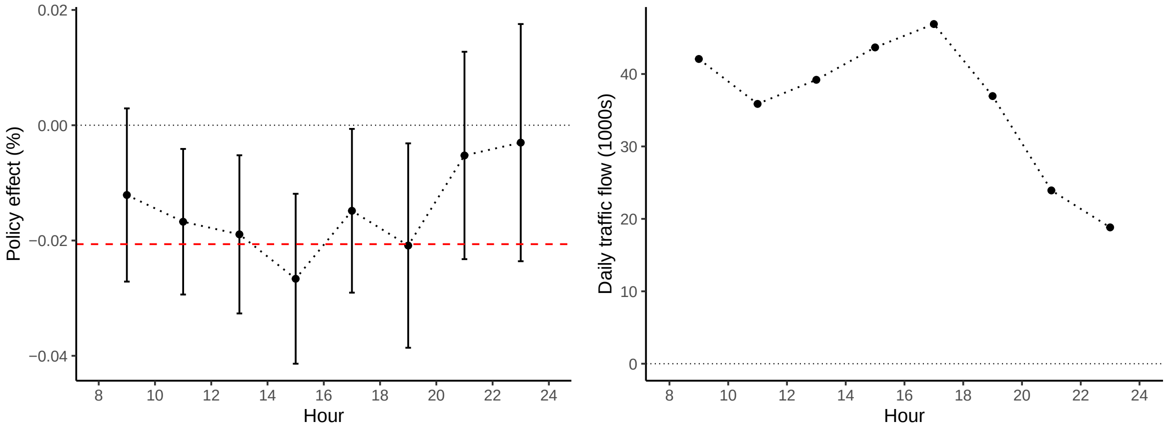


(b) Exits

Figure 2.B.6: Policy effect (percentages) on arrivals and exits within the day.

2.B.6 Heterogeneity on-street parking

The effects of the policy on the number of cars are equal to the policy effect in percentage terms multiplied by the average daily number of cars arriving or exiting per hour in the preceding year. Figure 2.B.6 shows that the policy effect on arrivals in percentages is relatively uniform up to 18:00 and becomes stronger in the evening, while the exit effect in percentages becomes stronger throughout the day. This indicates that motorists typically arriving and leaving in the evening are more price sensitive, which makes sense as parking is not work-related. This is confirmed in Figure 2.A.10, which indicates that over 90% of trips in the evening are non-work related while in the morning it is less than 50%. Furthermore, Figure 2.A.7 shows how daily on-street parking arrivals and exits vary over the day just prior to the policy. Notably, the peak in implied on-street traffic (the sum of arrivals plus exits) is highest around 15:00.



(a) Policy effect

(b) Traffic flow

Figure 2.B.7: Policy effect (percentages) and daily implied traffic flow within the day.

2.B.7 Welfare calculations

In this section we document how we calculate the back-of-the-envelope welfare effects presented in Section 2.4.3.1.

2.B.7.1 On-street market

Given the rule of half, the change in consumer surplus equals $\Delta CS = 0.5(Q + Q_n)(P_n - P)$. Plugging in values, we get €$326,000$ in the on-street market ($0.5(213,000 + 177,000)(4.22 - 2.55)$).

The change in government revenues equals to the revenues under the new policy minus the revenues under the old price scheme, so $\Delta R = P_n Q_n - PQ$. Plugging in values gives €$204,000$.

We assume that the new pricing policy is socially optimal, therefore the reduction in total social costs equals to the change in parking demand multiplied by marginal social cost (i.e. the opportunity costs of the land), which is (by assumption) the new price level. This equals $\Delta TSC = P_n(Q - Q_n) =$ €$152,000$.

Adding the government revenues, the change in total social costs and subtracting the change in consumer surplus gives the welfare gain of €30,000. This can also be calculated using the rule of half ($0.5 \times dP \times dQ$).

2.B.7.2 Off-street market

The change in consumer surplus in the off-street market equals: €$108,000$ ($0.5(106,000 + 101,000)(4.37 - 3.33)$).

Given that off-street parking is about half the size of the on-street market, 40% of off-street garage capacity is owned by the municipality, 20% is foreign-owned, and average prices off-street increased by around €1.00 we can calculate the change in profits as $\Delta\pi = P_n Q_n - PQ$ multiplied by the share of total capacity. Therefore, government revenues increase by about €35,000 (40% of total) and commercial operators gain €52,000 (60% of total), of which one third (€17,000) goes overseas.

In the off-street market, parking becomes idle and can not be replaced by other uses. Therefore the reduction in demand off-street is a welfare loss (note maximum capacity off-street during the day is generally below 60%). Hence reductions in demand off-street results in a welfare loss of €3,000.

2.B.7.3 Travel externalities

The change in travel externalities approximately equals the reduction in the number of car trips (arrivals effect times two) multiplied by the average distance of a trip within Amsterdam and the marginal passenger vehicle externality per km. Therefore $\Delta E_{vkt} = (2 \times 7,800 \text{ trips})(7 \text{ km})(€0.12 - €0.09) = €3,000$. This excludes additional externalities from road traffic occurring outside of Amsterdam. Non-residents tend to travel longer distances (36 km) which may reduce an additional 226,000 km ($(36 \text{ km} - 7 \text{ km})(2 \times 7,800 \text{ trips})$) of road traffic.

This will be an upper bound estimate if drivers travelling longer distances are less sensitive to price changes and a lower bound if travel externalities are larger in urban areas and motorists that do not use the car to travel to Amsterdam do not substitute to alternative destinations. One may also argue that congestion costs are overestimated because they are mainly incurred during peak hours. Our estimates from Figure 2.6 imply that around 39% of the reduction in traffic due to the policy occurs during peak hours, so this is unlikely to significantly effect our main estimates.

2.B.7.4 Cruising costs

Before the policy off-street parking was 30% (€0.76) more expensive than on-street parking and it was reduced to 10% (€0.36). Given an average parking duration of 2.4 hours, this implies that drivers were willing to pay up to €0.96 to avoid parking on-street and have to search for parking.

Taking the average VOT of car travellers in the Netherlands of €15.40, this roughly translates to travel time savings of around 4 minutes, which would increase welfare by around €74,000 per day ($€0.96 \times 77,000$ Arrivals). However, this is likely to be a large overestimate for four reasons. First, in a dynamic model, cruising does not occur in the morning when occupancies are still low. Second, we have ignored that the price differences between paid parking and discounted shopping areas which have increased because of the policy. Third, motorists may prefer off-street parking for reasons other than cruising. Finally, some off-street garages offer discounts for day parking, so this price difference (for longer durations) is likely to be smaller. Therefore it appears more reasonable that cruising costs are around one-quarter of the total amount (€18,000 per day).

2.B.7.5 Overall

Adding up the gains in the parking market (€27,000), the reduction in traffic externalities (€3,000), and the gains from less cruising (€18,000) implies an overall societal gain of around €48,000 due to the price increase.

As around half of all vehicle trips in the city are by residents, the gain to residents equals $\Delta W_R = 0.5(\Delta CS + \Delta E_{travel} + \Delta E_{cruising}) + \Delta R + \Delta TSC =$ €$195,000$. Meanwhile, the change in welfare for non-residents is negative and equals $\Delta W_N = 0.5(\Delta CS + \Delta E_{travel} + \Delta E_{cruising}) = -$€$196,000$. Given that there are around 850,000 inhabitants in Amsterdam, this suggests that the annual gains per resident are around $(195,000 * 365$ days$)/850,000 =$ €84.

2.B.8 Automated vehicles

In this section we document how we calculate the back-of-the-envelope implications for AVs presented in Section 2.4.3.2.

We consider the potential impact of AVs in the city centre (25,928 daily arrivals) and in peripheral areas (49,651 daily arrivals) where hourly parking prices are currently around €6.75 and €3.50, respectively. We assume that the price elasticities we estimate are symmetric. Furthermore, we assume that cars can park outside the city in designated parking areas for a fee of €2.50 per hour (the lowest price in the periphery) and that the cost to travel to and from this area is €2.00 from the city centre and €1.00 from the periphery. The hourly and fixed costs are divided by parking duration, given the new hourly prices, to come to an 'effective' hourly price.

We consider two scenarios for AVs. On the one hand, if households own private AVs and parking costs at the destination are sufficiently high, it is likely that AVs will be parked at locations in the periphery, where parking costs are relatively low. At these locations, parking costs will approximately equal the current on-street parking price in the periphery plus additional costs of travelling to and from the parking area. Therefore in the private AV scenario, we assume parking costs approximately equal current parking prices in the periphery, €2.50, plus the travel costs. On the other hand, if AVs are shared, then cars will only need to be parked during the evening and parking costs will be almost zero as they are shared between many users. Therefore, in the shared AV scenario, we assume that car users incur a small fee to hail a trip of €2.00.

2.B.8.1 Private AV scenario

We first consider the private AV scenario where effective hourly prices become €3.20 in the centre and €2.90 in the periphery. Effective hourly prices are equal to the hourly price outside the city plus the travel costs to and from the periphery divided by the number of hours. We first calculate the expected number of hours parked using the hourly price only.

Our estimates for the price elasticity of arrivals and duration imply that car demand by motorists currently paying hourly on-street parking will increase by about 14% (4,000 car trips and 0.4 hours) in the centre and 4% (2,000 car trips and 0.2 hours) in the periphery. As each additional trip results in one car travelling to and from the periphery to park, this corresponds to twice the amount of traffic generated by one parking trip. Taking our estimate for the effect on traffic flow, this then implies an increase in traffic of around 8% and 2%, respectively.

Given that about one-quarter of traffic is related to on-street parking, additional traffic generated by empty cars travelling to and from the periphery would result in substantially larger effects. Assuming that each trip needs to be made twice, this roughly translates to an overall increase in traffic flow of about 27% – 33%, in the periphery and centre, respectively.

2.B.8.2 Shared AV scenario

In the shared AV scenario, effective hourly prices become around €0.40 and are the same in the centre and in the periphery. Hourly parking prices are essentially zero, so the effective hourly price is composed only of the trip fee divided by the duration of the trip. In our to calculate the expected change in duration using our log-log model, we take €0.01 for the hourly parking price.

Our estimates then imply that car demand by motorists currently paying hourly on-street parking increases by around 55% (14,000 car trips and 2.9 hours) in the centre and 42% (21,000 car trips and 2.6 hours) in the periphery. This corresponds to an increase in traffic of around 16% and 12%, respectively.

3

Residential parking costs and car ownership

3.1 Introduction

Parking has far reaching consequences on urban life. In cities, where land is scarce, the opportunity cost of parking is high, as on-street spots compete with pedestrian, cycling, commercial, residential and recreational uses. Nevertheless, cities devote a substantial amount of space to implicitly subsidised parking which may induce excess vehicle demand (Shoup, 2005). This raises an important open question, *to what extent do parking costs affect vehicle demand in cities?*[52] We address this question by estimating residential parking costs and examining to what extent these costs affect household vehicle demand.

This chapter is based on Ostermeijer et al. (2019) as published in *Regional Science and Urban Economics*. We are grateful for suggestions and comments by seminar audiences in Amsterdam (Eureka), Dusseldorf (UEA), New York (UEA) and Hong Kong (ITEA) and two anonymous referees. A special thanks to Devi Brands, Jesper de Groote, Joris Klingen, Maurice de Kleijn, Paul Koster, Susan Ogilvie and Barry Ubbels for comments, programming help and discussions on earlier drafts. We also thank the NVM and Bisnode for providing data.

[52]Various other factors have been proposed to explain car ownership and car use in cities such as density, land use and accessibility. See, for example, Dargay (2002); Bhat and Guo (2007); Matas et al. (2009); Ewing and Cervero (2010) and Ding et al. (2017).

Theory indicates that cheap residential parking reduces the (fixed) costs of owning a car and thereby increases vehicle demand (Shoup, 2005; Arnott, 2006). The empirical literature that quantifies this effect is scarce, but supports the idea that higher residential parking supply and lower residential parking rents are associated with higher car ownership (Guo, 2013; Seya et al., 2016).[53] Furthermore, waiting time for an on-street parking permit is shown to negatively affect vehicle demand. Residents in Amsterdam that have to wait an additional year are 2 percentage points less likely to own more than one car, corresponding to a price elasiticity of demand for car ownership of -0.8 (De Groote et al., 2016).[54]

In order to estimate the impact of on-street parking costs on car ownership, one would like to observe market prices for on-street parking or close substitutes (for example off-street parking). In some countries, we are able to observe market rates for residential parking, as there is a thick rental market of privately-owned parking (for example Japan). However, in most countries, such a market is absent, as privately-owned parking is bundled with housing. Therefore, private off-street parking prices are not directly observed as residents mainly pay for parking through the purchase (or rental) of residential property or via regulated parking permits. Furthermore, in areas with excess demand, parking costs also include the time cost associated with cruising for parking.

This paper contributes to the literature on residential parking and car ownership by developing and applying a two-step approach which enables us to estimate local private parking costs and test to what extent these costs affect household car ownership.[55] In the first step, we identify the implicit price for parking through the effect of an outside private parking spot – arguably an almost perfect substitute for on-street parking – on house prices.[56] We exploit variation in the supply of private parking *within* a parking district to identify district-specific residential parking prices using semi-parametric hedonic house price methods.

Households considering car ownership face the same parking cost, on average, if they live in the same parking district. Hence, in the second step, we estimate the effect of residential parking costs on car ownership using variation in residential parking costs

[53] In their study for New York, Guo (2013) addresseses endogeneity issues by instrumenting parking variables using housing and demographic characteristics in the neighbourhood. However, these instruments can be criticised as these characteristics are determined by demand factors, so the exclusion restriction is not fulfilled. Seya et al. (2016) study the impact of residential parking rents in Japan, ignoring simultaneity issues.

[54] Average waiting times are around three years in the city centre of Amsterdam.

[55] In our application, local is defined as administrative parking districts.

[56] Parking comes in different forms. In our data we observe garages, carports and outside parking spots.

between districts. Endogenous parking costs are instrumented using the median construction year of properties in a district. Arguably, this instrument affects the supply of parking, while having no direct affect on parking demand, as it is determined in the past, often before cars were present.[57] We acknowledge that the construction year of properties is not random over space. Therefore, more precisely, we argue that, conditional on location controls, including, most importantly, distance to the nearest major train station, and household characteristics, historical supply decisions impact current building costs of a parking space, without directly affecting current demand for cars. We discuss this identifying assumption in more detail in the methodology section.

We focus on the Netherlands. In this context, residents who do not own private parking receive parking permits at very low fees and households with private parking are, in principle, not eligible for a parking permit. Hence, it is reasonable to assume that in equilibrium, *the residential parking price for households that own private parking is equal to the opportunity cost of parking on-street*, which equals the sum of the permit fee and cruising costs. The latter includes private search costs, walking time and uncertainty (Van Ommeren et al., 2011).[58] In case there is no cruising and street parking is not priced, residential parking prices should approach some underlying value of private parking, such as the security value or convenience of always having the car on hand. This approximately equals the value of private parking in locations where on-street parking is free.

We apply our approach to the four largest metropolitan regions in the Netherlands and estimate residential parking costs at the parking district level for owner-occupier households. On average, annual parking costs are around €1000 in city centres but are less than €400 in the urban periphery. We identify the impact of these costs on car ownership and find that owner-occupier households facing a one standard deviation increase (€503) in annual parking costs own 0.085 fewer cars on average, corresponding to a price elasticity of car demand of about -0.7. Our findings indicate that the disparity in parking costs between the city centre and the periphery explains around 30% of the difference in average car ownership rates between these areas.

Our results have implications for related literature on the urban spatial structure and transportation. Dense urban form is associated with lower vehicle ownership and kilometers travelled (Bento et al., 2005a; Bhat and Guo, 2007; Duranton and Turner, 2018). Furthermore, transport infrastructure has been shown to affect residential location and mode choice, however parking is usually ignored (Baum-Snow, 2007; Garcia-

[57]Supply-side instruments have also been used, for example, to investigate the effect of car ownership costs on the house price gradient in Singapore and housing supply elasticities in the US (Huang et al., 2018; Saiz, 2010).

[58]In waiting list districts, the implicit price also includes costs associated with waiting for a permit.

López et al., 2015; Baum-Snow et al., 2017; Levkovich et al., 2017; Heblich et al., 2018). Our findings shed light on one of the mechanisms which explains why car ownership levels are lower in dense urban areas and indicates that residential parking costs are a significant determinant of mode choice.

Our findings also relate to the growing literature on estimating the potential effects of automated vehicles (AVs).[59] We employ our estimates to consider the potential implications of raising fees of parking permits to the market value and eliminating parking costs from a widespread adoption of AVs. Increasing permit fees in the city centre of Amsterdam to the market value is expected to reduce average car ownership by 17 to 24 percent. Furthermore, the annual gains per household from facing lower parking costs are estimated to be between €450 and €850 in city centres, depending on whether AVs are private or shared. This is associated with an increase in average car demand between 8 and 14 percent. The effects are smaller in the periphery where parking costs are lower.

The paper proceeds as follows. In Section 5.2 we introduce the research context, data and provide some descriptives. In Section 5.3 we elaborate on the methodology. We report the main results in Section 5.4 and Section 5.5 concludes.

3.2 Data and context

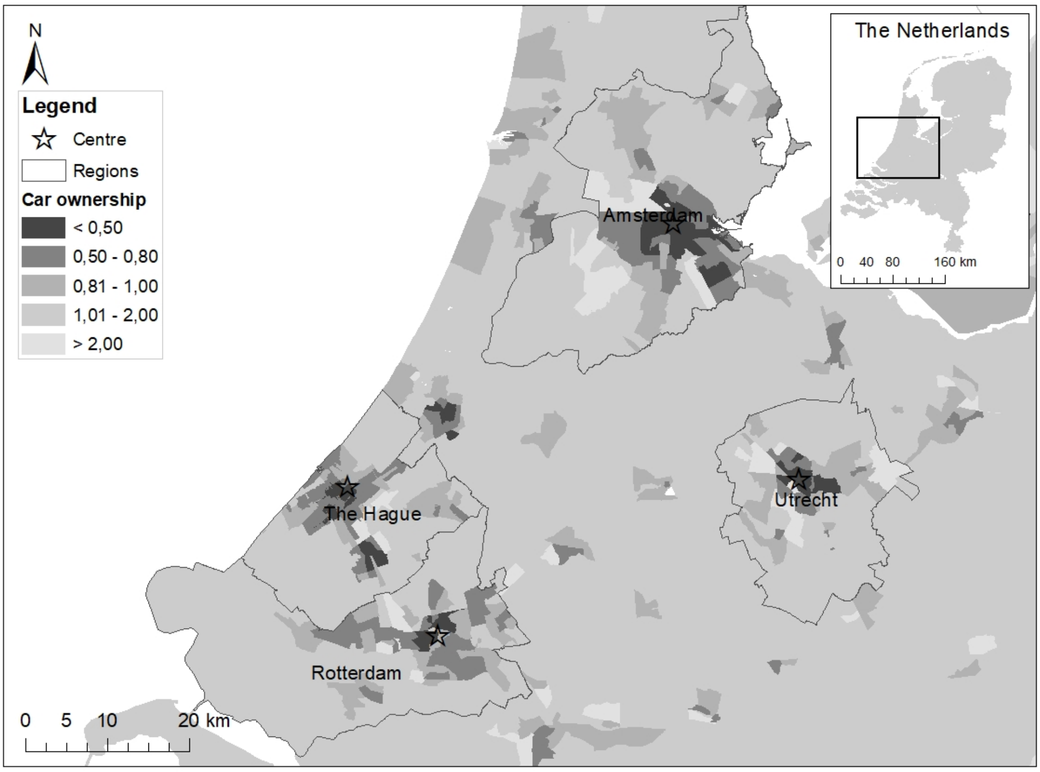
3.2.1 Parking and car ownership in the Netherlands

Dutch car ownership is low compared to most industrialised countries. Households own around one car on average, while in the UK and US they own around 1.5 and 2 cars, respectively (Clark and Rey, 2017). Moreover, in the Netherlands, as in other countries, car ownership is substantially lower in denser urban areas (see Figure 3.1).

Our methodology relies on house prices and therefore we focus exclusively on households that own a residence. In the Netherlands, around 95% of owner-occupiers own at least one car while only 30% also own a private off-street parking spot, so most owner-occupiers park their car(s) on-street. Regulation of parking has shifted over the last 30 years. In metropolitan areas, paid on-street parking was introduced in the early 1990s to tackle the growing problem of excess demand for parking. Currently, most dense urban areas have paid parking (see Figure 3.2). Due to scarcity of

[59] See for example Fagnant and Kockelman (2014); Childress et al. (2015); Zakharenko (2016); Gelauff et al. (2019).

Figure 3.1: Map of car ownership per household in the Randstad

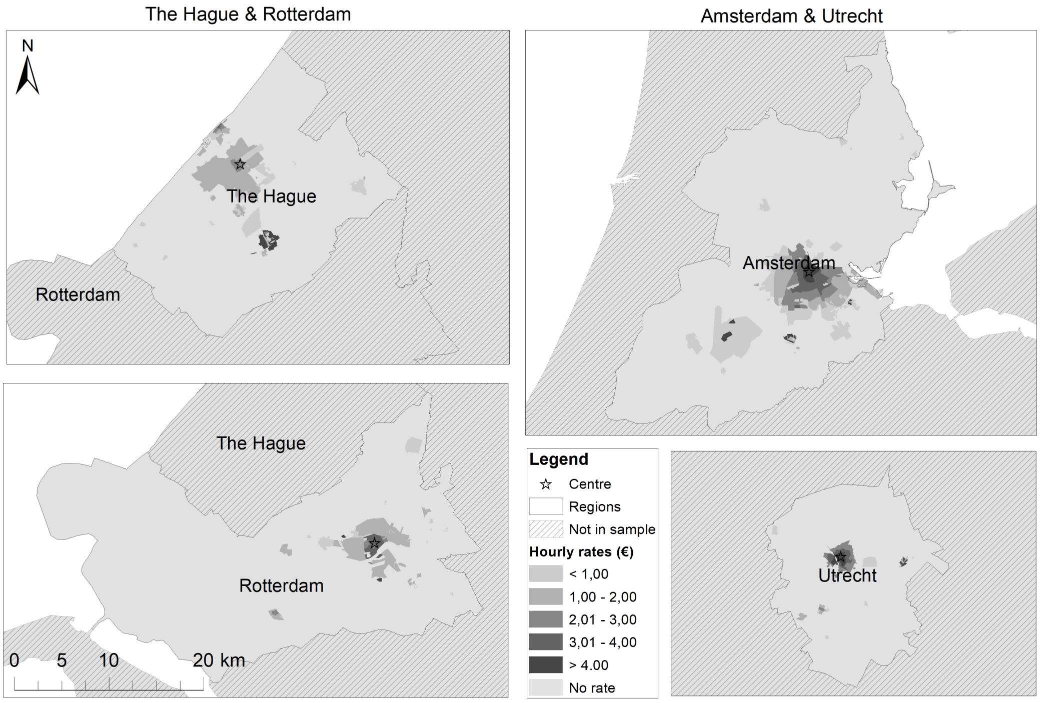


Note: The spatial unit is the four digit post code area.

land in these areas, there has been an ongoing policy shift towards discouraging car use through parking policy (Antonson et al., 2017). These policies include increases in parking prices for visitors, introducing parking permits and fees for residents, removal of on-street parking spots, lowering parking requirements for new buildings and developing fewer on-street spots (Mingardo et al., 2015; Gemeente Amsterdam, 2018b).

Parking policy is determined at the municipal level and on-street parking is almost owned entirely by local authorities. Policies are geared towards charging high hourly prices to visitors and providing residents with the option to apply for a permit. In contrast to many countries, including the US, where on-street parking is generally cheap, prices for on-street parking in the Netherlands are comparable to commercial off-street garages and can cost up to €5 per hour. Paid parking generally starts early, between 8:00-9:00, and ends late, between 18:00-23:59. Permits cost less than €100 per year, except in Amsterdam (see Table 3.1). Compared to visitor tariffs and commercial off-street parking, the daily permit fee is a fraction of the cost. For example, in the city centre of Amsterdam, permit fees are the highest in the country, but still only cost

Figure 3.2: Map of parking districts and hourly rates



Note: The rates in this figure refer to visitor tariffs for non-residents.

Table 3.1: On-street residential parking permit fees

	Amsterdam	Rotterdam	The Hague	Utrecht
Permit fee (€/yr)				
Centre (<2 km)	500	70	40	70
Urban ring (2-5 km)	200	70	40	30
Periphery (>5 km)	0	0	0	0

Notes: Fees are rounded averages for the areas indicated in 2018. During the period of study, 2000-2016, fees where lower.

€1.40 per day, while an identical on-street spot costs visitors around €45 per day. Therefore, as its costly, on-street parking without a permit is not a realistic option for most residents.

Residents with a car can choose to apply for an on-street parking permit except when they live in a property with private parking.[60] Depending on the location, households can apply for one or two parking permits. In Amsterdam almost all inner city locations allow only one permit and in some areas in the centre residents need to wait several years before obtaining a permit (De Groote et al., 2016). All metropolitan areas have good transport alternatives to the private car. These include a high quality public transport system of buses, trams, trains and in the case of Amsterdam and Rotterdam, a metro system. Furthermore cycling usage in cities is high, around 35% of all trips within 7.5 km are on the bike (Rietveld and Daniel, 2004).

3.2.2 Data

We use three main datasets. In the first step we use transaction data on houses from the Dutch Association of Real Estate Agents (NVM). The dataset contains around 80% of all residential property transactions in the Netherlands between 2000 and 2016 and is recorded at a highly detailed level. It includes location coordinates for each unit, structural, historical and qualitative housing characteristics and transaction details. This data allows us to estimate private residential parking costs in the first step. We match the property data to administrative parking districts and select housing transactions within the four largest metropolitan regions of the Netherlands.[61] On average, each district has approximately 2000 properties, so parking districts are small. We remove districts with few observations and exclude large outliers from the remaining

[60] Renting a parking spot, for example from a private company, occurs seldom and prices of these parking spots reflect implicit parking prices paid for residential parking (Van Ommeren et al., 2011).

[61] Peripheral areas generally do not have paid parking (see Figure 3.2). Therefore, in these areas we designate four digit post code units as parking districts.

dataset.[62] After selections, the transactions dataset contains a total of 535,097 observations.

In the second step, we obtain household information from Bisnode and current building registry information from Building Characteristics Netherlands (GKN). Bisnode is a marketing firm that carries out representative surveys of households around the Netherlands, of which we have data between 2004 and 2014. The dataset distinguishes between zero, one and two or more cars per household.[63] Household location is precisely measured at the six digit post code (PC6) level, which contain around 20 properties on the same side of the street. Household characteristics include income, size, type, education, age and home-ownership status, which we use to select adult owner-occupiers.[64] We use the GKN dataset to construct geographical variables including the median construction year of residential properties in a parking district and building density in a PC6 area.[65] Finally, we also measure proximity to transport infrastructure and the city centre by calculating the distance from each PC6 area to the nearest train station, highway, highway ramp and metropolitan city centre. The availability of public transport is measured by the number of bus, metro and train stations within 100, 250 and 500 meter buffers of the PC6 centroid.

3.2.3 Descriptives

Table 3.2 presents the main descriptive statistics for property transactions. The average transaction price is around €230,000, average size of a property is around 100 m^2 and the majority of properties are apartments (56%). Around 20% of properties have off-street private parking of which almost a third are outside, one fifth are semi-sheltered carports, half have a garage structure and very few have space for two cars.

Table 3.3 provides an overview of the main household characteristics. We have information about 98,659 owner-occupier households in 493 geographically distinct park-

[62]We select districts with at least 10 transactions of houses with outside parking and 10 transactions of houses without outside parking. This is explained in more detail in Section 3.3.1. Outliers are determined to be transactions above €2.5 million, €5,000/m^2 property size, €5,000/m^2 parcel size, 500 m^2 parcel size, 250m^2 property size and 25 rooms. Similarly, we remove observations below €25,000, €500/m^2 property size, €400/m^2 parcel size, 50 m^2 parcel size and 40 m^2 property size.

[63]Only 4.2% of households own three or more cars in the Netherlands (CBS Statline, 2015). This is likely to be much lower in the metropolitan areas we focus on. Therefore, any measurement error from not observing the exact number of cars is negligible.

[64]Income is measured at the household level, while education and age is for the household head.

[65]The median construction year is truncated at 1900 as there is little variation in parking supply before 1900.

Table 3.2: Descriptive statistics: Main transaction variables

	Mean	Std. dev	Min	Max
Transaction price (€)	227,923	111,246	26,092	1,200,000
Size of property (m^2)	104.88	35.66	41	249
Size of parcel (m^2)	168.10	77.43	51	499
Distance city center (km)	7.06	5.13	0	29
Apartment	0.56	0.50	0	1
Private parking	0.20	0.40	0	1
Outside	0.07	0.25	0	1
Carport	0.04	0.20	0	1
Garage	0.09	0.28	0	1
Carport & garage	0.00	0.06	0	1
Double garage	0.01	0.09	0	1
# Transactions	535,097			

Note: We only observe parcel size for single family homes (234,395 observations). See Appendix A for a full list of variables.

ing districts. The average household in the sample owns 1.2 cars, has an annual income of €46,000 and consists of 2.8 members.[66] Around 60% own one car, 30% own two or more cars, whereas few do not own a car (around 10%). Households live farther from the city centre than in the transaction dataset, 9 km vs 7 km and around 30% of households are apartment dwellers.[67] Most households live in highly built up urban areas, average building density is 33,700 m^2/ha, and the median construction year of properties is 1966.

3.3 Empirical methods

We develop a two-step methodology to estimate the effect of residential parking costs on car demand. In the first step we use hedonic house price methods to estimate implicit market prices for parking. To be more precise, we focus on local implicit prices for private outside parking spots which is a close substitute to on-street parking. In equilibrium, private parking prices should reflect (unobserved) outside parking costs. In the second step we investigate the effect of these prices on car ownership.

[66] Owner-occupiers tend to own more cars, are richer and have more individuals than an average Dutch household which owns 1 car, earns €45,000 and is composed of 2.2 people (CBS Statline, 2015).

[67] This is less than in the transactions data, as apartments are generally sold more frequently than single-family houses.

Table 3.3: Descriptive statistics: Main household variables

	Mean	Std. dev	Min	Max
Number of cars	1.20	0.62	0	2
No car	0.11	0.31	0	1
One car	0.58	0.49	0	1
Two or more cars	0.31	0.46	0	1
Income (€)	46,052	21,954	15,178	120,750
Education low	0.27	0.44	0	1
Education middle	0.37	0.48	0	1
Education high	0.36	0.48	0	1
Age	46.75	15.00	18	90
Household size	2.80	1.24	1	6
Apartment	0.31	0.46	0	1
Distance city center (km)	9.03	5.41	0	29
Within historic district	0.05	0.21	0	1
Building density (m^2/ha)	33,743	26,820	523	190,895
Median construction year	1966.33	21.85	1900	1999
# Households	98,659			

Note: See Appendix A for a full list of variables.

3.3.1 Step 1: Estimating parking costs

Our methodology exploits variation in the allocation of private parking *within* a parking district to identify district-specific residential parking costs using hedonic house price methods. As a household is only eligible for a parking permit when no private parking is available, spatial equilibrium theory predicts that for household utility to be the same in a given district, the implicit residential parking price should equal the costs of using a permit, i.e. the sum of the permit fee and cruising costs, capitalised in house prices (Van Ommeren et al., 2011).

We identify the implicit price of parking defined by the effect of having a private parking spot on house prices. Let us start with the following, naive, hedonic price regression:

$$P_{ijt} = \rho S_{ijt} + \mathbf{T}_{ijt}\alpha + \phi_t + \epsilon_{ijt}, \tag{3.1}$$

where P_{ijt} is the price for residential property transaction i in parking district j at time t, S_{ijt} is an indicator variable which equals one if the property has a private parking spot and zero otherwise. We also include four parking type dummies, $\mathbf{T}_{ijt}$, for carport, garage, carport and garage and double garage, which captures additional value of the building structure. Therefore, ρ can be interpreted as the implicit price (or cost) for a private outside parking spot. Lastly, ϕ_t is a vector of time fixed effects and ϵ_{ijt} is the error term.

We are interested in the causal effect of S_{ijt}, captured by ρ. It is unlikely that the estimate of ρ in (3.1) generates a causal estimate. For example, districts have different parking policies and may be attractive to car users for other reasons. Therefore we include parking-district fixed effects, ϕ_j, which absorb differences between parking districts and allow us to identify parking costs via variation *within* a parking district.[68] Moreover, there may be other housing or locational characteristics within a district that are correlated to property prices and parking allocation. For example, bigger properties are generally more expensive and are also more likely to have a parking spot. Hence we control for a large set of property and locational characteristics, $\mathbf{X}_{ijt}$.[69]

Parking costs are likely to vary locally, because supply and demand factors vary over space, so we allow ρ to vary at the parking district level j. Furthermore, the implicit price for housing and locational characteristics, as well as changes in property prices over time, are also likely to vary over space and may be correlated to local parking allocation.[70] Therefore we also allow the effect of housing and location characteristics $\mathbf{X}_{ijt}$ and time dummies ϕ_t to vary with j. This leads to the following regression:

$$P_{ijt} = \rho_j S_{ijt} + \mathbf{T}_{ijt}\alpha_j + \mathbf{X}_{ijt}\gamma_j + \phi_{jt} + \phi_j + \epsilon_{ijt}, \tag{3.2}$$

where the coefficients ρ_j, α_j and γ_j represent the implicit price for parking, the associated structure and other housing characteristics, respectively. The interaction ϕ_{jt} captures a district-specific time fixed effect.[71]

[68]This implies that we can only identify ρ in districts where there is variation in the supply of *outside* parking.

[69]Property characteristics include; the log of size and parcel size (for single-family houses), the number of floors, rooms and bathrooms, and dummies for garden, balcony, central heating, new, monument, good inside maintenance, good outside maintenance, insulation (five levels) transaction year, construction year (nine interval dummies) and house type (apartment, terraced, detached, semi-detached, corner). Location characteristics include; distance to the metropolitan centre, closest train station and closest highway ramp and are specified in logs.

[70]For example, an additional meter of house size is likely to have a higher implicit price in the city centre than in the periphery as the demand for space is higher and the supply is fixed in the historic central part of most cities. This may lead to less allocation of parking as the space could be used for more valuable uses.

[71]As we perform local linear regression, a linear form for the dependent variable is preferable because implicit prices do not directly depend on average house prices.

The coefficients, ρ_j, α_j and γ_j can be estimated by interacting S_{ijt}, $\mathbf{T}_{ijt}$ and $\mathbf{X}_{ijt}$ with parking district dummies. However, as most districts have few observations and there are many coefficients to be estimated, the variance of the implicit price estimates are high and outliers can lead to considerable variation in ρ_j (McMillen and Redfearn, 2010). To tackle this issue, we assume that districts neighbouring j have similar implicit prices as j which can be used to reduce the estimates' variance.[72] A semi-parametric approach can then be applied where neighboring parking districts receive a higher weight than distant districts. To be more precise, we use the distance between the centroid of each parking district j, and any other district.[73] We estimate a partially linear regression model:

$$P_{ijt} = f_j(S_{ijt}, \mathbf{T}_{ijt}, \mathbf{X}_{ijt}, \phi_t) + \phi_j + \epsilon_{ijt}, \tag{3.3}$$

where the function $f_j(\cdot)$ is estimated in a non-parametric way. We estimate $f_j(\cdot)$ by locally weighted regression, implying:

$$f_j(\cdot) = \rho_j(u_j, v_j)S_{ijt} + \mathbf{T}_{ijt}\alpha_j(u_j, v_j) + \mathbf{X}_{ijt}\gamma_j(u_j, v_j) + \phi_t(u_j, v_j),$$

where (u_j, v_j) are the centroid coordinates of parking district j. The district-specific implicit parking costs ρ_j are then defined by $\rho_j(u_j, v_j)$.

Locally weighted regression techniques have been extensively used in the hedonic house price literature where parameters depend on geographic location (Sunding and Swoboda, 2010; Grislain-Letrémy and Katossky, 2014). For each parking district j, we estimate a weighted least squares (WLS) regression using an exponential distance decay kernel:

$$w_{jk} = \begin{cases} e^{-hd_{jk}}, & \text{if } d_{jk} < 5 \\ 0, & \text{otherwise,} \end{cases} \tag{3.4}$$

where w_{jk} is the weight applied to all property transactions in parking district k, the

[72] To prevent estimating ρ_j for districts without any outside private parking, we select districts with at least 10 transactions with outside private parking and 10 transactions without outside private parking.

[73] To speed up the estimation, we set 5 km as the cutoff point. Therefore the weight for any observation i in parking district k greater than 5 km from j is set to zero. This does not materially effect the estimates of ρ_j as weights are approximately zero after 5 km (see Figure 3.A.1 in Appendix 1).

bandwidth h determines the speed of the decay and d_{jk} is the euclidean distance, in km, between the centroids of parking district j and k. Figure 3.A.1 in Appendix 1 illustrates the weighting function using various distance decay bandwidths. In our application, we estimate these partially linear regression models for each region separately as this makes the estimation procedure faster.[74]

As a part of the model is parametric, specifically the district fixed effect ϕ_j, we use a two-step estimation procedure (Bontemps et al., 2008). In the first step, the linear part of the specification, can be estimated using the Robinson (1988) approach. This method separately regresses P_{ijt} and the parametric part ϕ_j on the non-parametric part $f_j(\cdot)$ using local WLS and generates residuals, $\widetilde{P_{ijt}}$ and $\widetilde{\phi_j}$. The residuals $\widetilde{P_{ijt}}$ are then regressed on the residuals $\widetilde{\phi_j}$ using OLS and the coefficients $\hat{\zeta}$ on $\widetilde{\phi_j}$ are captured.[75] In the second step, we then regress $P_{ijt} - \hat{\zeta}\phi_j$ on the non-parametric part $f_j(\cdot)$ using local WLS to get the parking district specific coefficients of interest.

An important parameter in non-parametric estimation is the bandwidth. A lower bandwidth implies more bias, but lower variance, as the estimates are smoothed more over space. Meanwhile, a higher bandwidth implies less smoothing, therefore less bias and higher variance.[76] We will use a bandwidth of $h = 2$, which allows for a sufficient amount of variation in the estimates over space, while also having a variance that is economically meaningful. In Section 5.4 we show that lower bandwidths provide larger estimates of the price elasticity of car demand, so our approach is somewhat conservative, while higher bandwidths provide unrealistic estimates (for example negative parking costs and many large outliers).[77]

It is important to discuss the interpretation of the estimated implicit price, $\widehat{\rho_j}$. As parking is a discrete variable, those that own a private spot are willing to pay *at least* $\widehat{\rho_j}$, while households that do not own a private spot are *maximally* willing to pay $\widehat{\rho_j}$ (Bajari and Kahn, 2005).[78] Hence, we interpret the implicit price of a private outside parking spot as the (average) cost for an outdoor parking space for all residents living in district j.

[74] This provides essentially the same results as estimating all regions simultaneously as most regions are more than 5 km apart, therefore observations of other regions are given a weight of zero.

[75] We estimate $\hat{\zeta}$ by regressing: $\widetilde{P_{ijt}} = \zeta\widetilde{\phi_j} + u_{ijt}$. Under regularity conditions, Robinson (1988) shows that the coefficient is a $\sqrt{n}$-consistent and asymptotically normal estimator for $\hat{\zeta}$.

[76] A bandwidth of $h = 0$ implies each observation gets $w_{jk} = 1$ and we are back to specification (3.1), including more controls, where parking costs are assumed to be constant over space (causing high bias). A bandwidth of $h = \infty$ implies that we do not take into account the spatial correlation in ρ_j's as in specification (3.2).

[77] We detect any remaining large outliers as greater than or smaller than $mean(\widehat{\rho_j}) \pm 4 * std(\widehat{\rho_j})$.

[78] The Bajari and Kahn (2005) approach has also been used to study car ownership, see for example Mulalic and Rouwendal (2015).

3.3.2 Step 2: Parking costs and car demand

In the second step, we aim to estimate the effect of residential parking costs on vehicle demand. As mentioned in Section 3.3.1, implicit parking prices reflect parking costs, which are assumed to be the same for all households within a parking district. The identification strategy exploits spatial variation in implicit residential parking costs *between* parking districts to explain household vehicle demand using a multinomial logit (MNL) model. The MNL model assumes a random utility framework with k alternatives and i individuals, living in district j at time period t. As utility is not directly observed, we construct a model:

$$U_{ijt}^k = \lambda_{ijt}^k + \epsilon_{ijt}, \tag{3.5}$$

where the unobserved utility derived from alternative k, U_{ijt}^k, is composed of a deterministic component, λ_{ijt}^k, and a random component, ϵ_{ijt}, which is independently and identically distributed across alternatives with an Extreme Value Type I distribution. Therefore, the probability a household owns $C_{ijt} = k$ cars, where $k = 0, 1, \geq 2$, can be written as:

$$Pr[C_{ijt} = k] = \frac{e^{\lambda_{ijt}^k}}{\sum\limits_{\widetilde{k}=0}^{2} e^{\lambda_{ijt}^{\widetilde{k}}}}. \tag{3.6}$$

We are mainly interested in how residential parking costs, $\widehat{\rho_j}$ affect the probability of owning k cars. Therefore, we specify the deterministic part λ_{ijt}^k as:

$$\lambda_{ijt}^k = \beta^k \widehat{\rho_j} + \phi_t^k, \tag{3.7}$$

where we control for year fixed effects, ϕ_t^k, and the error term is clustered at the parking district level, j, as parking costs are at a more aggregate level than household car ownership. The reference category is $k = 0$ cars, so we set $\beta^{k=0} = 0$.

One concern with equation (3.7) is that households with a higher (lower) preference to own a car may sort into areas with lower (higher) parking costs. Therefore β^k will be overestimated as parking costs and household characteristics related to vehicle demand are correlated. For example, larger families may want to own more than one car or live in a larger house and therefore choose to locate outside the densest areas in cities where parking costs are lower. Furthermore some households may have strong preferences for car ownership or urban amenities. Therefore, we control for household characteristics, $\mathbf{H}_{ijt}$, which include income, age, size, type and education.

The availability of substitutes and the ease of using a car may also correlate with vehicle demand and parking costs, so we add location characteristics, $\mathbf{L}_{jt}$, which include distance to transport infrastructure, the availability of public transportation, distance to the city centre, whether the household lives in a historic district and building density. Controlling for distance to the city centre is particularly important as it captures the stylised fact that in European cities, urban amenities are highly correlated with distance to the city centre and therefore may also be correlated to preferences for car ownership and residential parking costs. Finally, as parking policy is determined at the municipality level, we include municipality fixed effects, ϕ_m^k, which also controls for other local unobserved characteristics of the built environment such as land use regulations that may influence vehicle demand and parking costs. This would suggest the following specification:

$$\lambda_{ijt}^k = \beta^k \widehat{\rho_j} + \mathbf{H}_{ijt}\gamma^k + \mathbf{L}_{jt}\theta^k + \phi_m^k + \phi_t^k. \tag{3.8}$$

A major concern with (3.8) is that because residential parking costs are determined by supply and demand for parking, vehicle demand will be correlated to parking costs. Therefore, the specification suffers from reverse causality and the estimated coefficient β^k is inconsistent. We attempt to solve this problem by instrumenting $\widehat{\rho_j}$ using the median construction year of properties in a district, B_j, similar to Van Ommeren et al. (2012). The median construction year of properties is a conditionally-valid instrument as it affects current parking costs via historical supply restrictions, reflecting historical land and building costs. Therefore the main assumption for identification is that, conditional on household and location controls, the median construction year of residential properties in area j only affects current vehicle demand *via* historical supply factors and is uncorrelated to the *current* demand for parking in area j.

It may be the case that households with preferences for car ownership sort into parking districts with newer buildings and lower costs. We argue that this is a minor threat to our identification as the lion's share of sorting is likely controlled for by the detailed set of housing characteristics, $\mathbf{H}_{ijt}$, and distance to the city centre. Furthermore, we exclude parking districts with a median construction year after 1999 and exclude households living in properties constructed after 1999 as parking costs in newer districts and houses may be affected by current parking demand. It is important to note that in the first-step the implicit parking costs are estimated conditional on construction year of the property, so the cost should not be influenced by its own construction year.

There are two additional advantages of instrumenting for $\widehat{\rho_j}$, compared to using a standard MNL model. Firstly, because we identify the impact of changes in parking costs due to a shift in supply, conditional on controls, we address the issue that random measurement error is introduced during the estimation of costs in step 1 which usually causes a downward bias in the estimated β^k coefficient. Secondly, it mitigates issues from any other omitted factors, correlated to vehicle demand and parking costs.

As a MNL model is non-linear in parameters, 2SLS estimators are inappropriate, so we apply a control function approach (Petrin and Train, 2010; Wooldridge, 2015). In the first stage we estimate:

$$\widehat{\rho_j} = \eta B_j + \mathbf{H}_{ijt}\gamma + \mathbf{L}_{jt}\theta + \phi_m + \phi_t + \upsilon_j, \tag{3.9}$$

where B_j is the median construction year of residential properties in parking district j and υ_j is the residual. In the second stage, we plug in $\hat{\upsilon}_j$ linearly as an additional control and specify:

$$\lambda^k_{ijt} = \beta^k \widehat{\rho_j} + \mathbf{H}_{ijt}\gamma^k + \mathbf{L}_{jt}\theta^k + \phi^k_m + \phi^k_t + \hat{\upsilon}_j, \tag{3.10}$$

where standard errors are bootstrapped (250 replications) over both steps and clustered at the parking district level j.

The parameters of a MNL model represent the probability one alternative is chosen as compared to the base category. Therefore, the directionality and magnitude of the coefficients are not straightforward to interpret. In light of this, we calculate and present the average marginal effect (AME) for the variables of interest on the choice probabilities of each car ownership alternative (Bhat and Pulugurta, 1998). The marginal effect of a continuous variable, for example parking costs $\widehat{\rho_j}$, on the probability a household i chooses k cars, $\pi^k_{ijt} = Pr[C_{ijt} = k]$, can be written as:

$$\Delta Pr[k] = \frac{\partial \pi^k_{ijt}}{\partial \widehat{\rho_j}} = \pi^k_{ijt}(\widehat{\beta}^k - \sum_{\widetilde{k}=0}^{2} \pi^{\widetilde{k}}_{ijt}\widehat{\beta}^{\widetilde{k}}). \tag{3.11}$$

We take the average of the marginal effects over all households to get the AME, denoted as $\Delta Pr[k]$. Using the AMEs, we can calculate the change in average car ownership as:

$$\Delta E[C] = 1 \cdot \Delta Pr[1] + 2 \cdot \Delta Pr[2]. \tag{3.12}$$

When estimating a MNL model, one does not impose restrictions on the marginal effect of a variable on the probability an alternative is chosen. If parking costs have a larger impact on the demand for a second car because it is for example less essential than the first car for mobility, we can test whether the effect of parking costs varies over each car ownership alternative. When the AME on $k = 1$ car, $\Delta Pr[1]$, is zero, it indicates that the number of households switching from $k = 1$ to $k = 0$ cars is the same as from $k = 2$ to $k = 1$ cars. This suggests that the assumption of a linear restriction holds and therefore, we can apply linear regression techniques, such as 2SLS, which are more efficient. We therefore also estimate 2SLS models.

3.4 Results

In this section we present the results from estimating implicit parking costs (Section 3.4.1), the impact of these costs on household vehicle demand (Section 3.4.2) and additional sensitivity checks (Section 3.4.3).

3.4.1 Step 1: Estimating parking costs

In Table 3.4 we present the average implicit parking prices, or costs, of various parking types for each region obtained by estimating equation (3.3). The implicit parking price can be interpreted as the average price for a private outside parking spot. This represents the net present value of future benefits from private parking as compared to parking on-street with a permit. The average price for an outside private parking space is around €12,000 and is highest in the Amsterdam region. Prices are generally higher for parking spaces with structure, such as garages, and for larger lots which suggests higher construction costs and other uses such as storage.[79] Prices

[79] Implicit prices for large parking spots are based on few observations (see Table 3.2), therefore estimates are less precise (have large standard errors) and should be interpreted with caution. A priori, it is not clear whether carports should be cheaper or more expensive than garages as carports may have space for more than one car while garages include a physical structure. Our findings indicate that garages are more expensive than carports in general, however carports seem more valuable in Utrecht.

Table 3.4: Average implicit parking costs (€)

	Amsterdam	Rotterdam	The Hague	Utrecht	Overall
Private (outside) parking	14147	12747	9264	10422	11914
	[12825]	[11010]	[9992]	[8089]	[11188]
Carport	18353	15816	17996	20200	17990
	[13990]	[15485]	[16336]	[15419]	[15281]
Garage	21384	18683	20486	10851	18788
	[10051]	[8542]	[9689]	[9310]	[10194]
Carport & garage	21042	27973	31542	14569	24331
	[21242]	[26953]	[30522]	[23034]	[26282]
Double garage	22651	29765	16886	10907	20781
	[27077]	[27730]	[25906]	[15945]	[26173]
# Transactions	182,958	121,128	142,303	88,708	535,097

Notes: Costs are a representative average for all transactions over the time period 2000-2016. Standard deviation in brackets. Full table of implicit prices are available upon request.

Table 3.5: Average annual implicit outside parking costs (€/yr)

	Amsterdam	Rotterdam	The Hague	Utrecht	Overall
Overall	707	637	463	521	596
	[641]	[550]	[500]	[404]	[559]
Centre (<2 km)	1609	826	953	771	1023
	[215]	[181]	[347]	[422]	[463]
Urban ring (2-5 km)	1060	1122	535	420	792
	[513]	[669]	[365]	[354]	[552]
Periphery (>5 km)	354	476	322	455	395
	[481]	[475]	[554]	[370]	[487]
# Parking-districts	147	141	155	99	542

Notes: Costs are a representative average for all parking districts, weighted by the number of transactions in a parking district, over the time period 2000-2016. Standard deviation in brackets.

vary slightly between regions which suggests different supply and demand conditions. As we are interested in estimating the effect of parking costs on car ownership, we derive annual parking costs by assuming zero depreciation costs and an annual discount rate of 5%.[80] Hence, we multiply the implicit price $\widehat{\rho_j}$ by 0.05.

Table 3.5 presents the average annual implicit outside parking costs. There are a total of 542 parking districts in the sample. On average, annual parking costs are around €600 and seem to follow an approximately normal distribution (see Figure 3.B.1 in

[80]Outside parking is unlikely to depreciate as it does not include any building structure. This discount rate gives realistic parking cost estimates, as discussed at the end of this section.

Figure 3.3: Map of annual residential parking costs (€/yr)

Appendix B). Around 13% of the estimates are negative, most of which are close to zero and statistically insignificant. Furthermore, another 21% of the estimates are positive and not significantly different from zero (see Table 3.B.1 in Appendix B). Hence, for about one third of the estimates, parking costs are essentially zero. This makes sense as outside parking costs are close to zero in peripheral areas.

We also separate the results by distance to the metropolitan centre and present the results graphically. Table 3.5 and Figure 3.3 show that there is substantial heterogeneity in annual parking costs over space with higher costs generally in central city areas, especially in Amsterdam where annual costs are around €1600 within 2kms from the city centre and fall with distance. Costs in the periphery level off at around €300 to €500. Parking costs in Rotterdam are slightly different from the general trend and are higher surrounding the city centre. This is likely because the city centre of Rotterdam was re-built after the bombings in WWII and therefore has a higher supply of parking than the historic neighbourhoods surrounding the centre (Koster et al., 2012). Overall, the estimated implicit parking costs appear realistic.[81]

[81]Based on current list prices from Funda, the largest online multi-listing housing market platform in

Table 3.6: Main results

	MNL		MNL-CF		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Parking cost (€100/yr)						
Pr[0 car]	0.00356***	0.00120***	0.0126***	0.00597**		
	(0.000548)	(0.000351)	(0.00179)	(0.00241)		
Pr[1 car]	-0.00170**	-0.00131**	0.00516**	0.00269		
	(0.000682)	(0.000597)	(0.00210)	(0.00379)		
Pr[2 cars]	-0.00186**	0.000105	-0.0177***	-0.00866*		
	(0.000929)	(0.000606)	(0.00331)	(0.00501)		
$\Delta E[C]$	-0.005***	-0.001	-0.030***	-0.015**	-0.035***	-0.017***
	(0.001)	(0.001)	(0.005)	(0.007)	(0.005)	(0.005)
ε_P^C	-0.23***	-0.05	-1.26***	-0.61**	-1.45***	-0.70***
	(0.06)	(0.03)	(0.20)	(0.29)	(0.21)	(0.20)
Controls (18)	N	Y	N	Y	N	Y
Year FE's (10)	Y	Y	Y	Y	Y	Y
Mun FE's (45)	Y	Y	Y	Y	Y	Y
First stage F-statistic			51.55	15.64	65.68	22.00
# Parking-districts	493	493	493	493	493	493
# Households	98,659	98,659	98,659	98,659	98,659	98,659

Notes: Dependent variable is the number of cars per household. Standard errors are in parenthesis and are clustered at the parking-district level. For MNL specifications, AMEs and two-stage clustered bootstrapped standard errors and (Kleibergen-Paap) First stage F-statistics (250 replications) are presented. MNL-CF refers to MNL model with a control function approach. $\Delta E[C]$ represents the change in average car ownership from a €100 increase in parking costs and is calculated as in equation (3.12). See Appendix 2 for calculation of ε_P^C, the implied price elasticity of car ownership, standard errors are calculated using the delta method. See Tables 3.B.2 and 3.B.3 for full table with controls and first-stage regression results. Stars denote * p < 0.1, ** p < 0.05, *** p < 0.01.

3.4.2 Step 2: Parking costs and car demand

The maps of car ownership and parking costs in Figures 3.1 and 3.3 suggest that there is an inverse relation between vehicle ownership and residential parking costs. This relation is investigated in more depth in this section. Table 3.6 presents the main results.

Firstly, in columns (1) and (2) we present the average marginal effects (AME) from

the Netherlands, rental prices in 2019 for private parking spots in city centres are around €3000 in Amsterdam and are around €1500 in Rotterdam, The Hague and Utrecht (Funda, 2019). Note, these prices are not directly comparable as housing prices have almost doubled since 2008 (the average year in the data), private rental spots are generally garages which are more expensive and because implicit prices should be lower than market prices due to permits.

estimating specification (3.7) and (3.8) using MNL, hence we still ignore a range of endogeneity issues. We see that there appears to be a small, negative effect of parking costs on car ownership, with a smaller effect size when controlling for household and location characteristics. These results are however difficult to interpret as causal estimates because reverse causality and measurement error will likely bias the estimates towards zero. Owning a vehicle creates demand for parking and thereby raises prices, resulting in a positive bias in the coefficient for parking costs on vehicle demand, whereas if there is (random) measurement error in step 1, results will be biased towards zero.

In columns (3) and (4) we estimate specification (3.10), using a MNL control function approach, where parking costs are instrumented with the median construction year of buildings in a parking district. It is useful to discuss the sign of the instrument. In the Netherlands, car ownership has grown over the last century. Hence, we expect that, ceteris paribus, parking supply will be higher in areas where buildings have been constructed more recently, as parking does not need to be re-developed from pre-existing land uses which is costly in built-up areas. Therefore we should see a negative relation between the median construction year of residential properties in a parking district and parking costs. Results from the first stage show that the instrument is strong, the Kleibergen-Paap First stage F-statistic is 51.55 and 15.64, respectively, and indeed has the expected negative sign.

The results from column (4) indicate that the AME of residential parking costs on the probability of owning one car is zero, while for the second car it is negative. This indicates that as parking costs increase, around the same number of households switch from two to one car as the number that switch from one to zero cars, implying that the effect of parking costs on vehicle demand is approximately linear and therefore 2SLS can be applied. Comparing the effect of parking costs on average car ownership, $\Delta E[C]$, with and without control variables in columns (3) and (4) suggests that controlling for household and location characteristics are important for the conditional validity of the instrument.

In columns (5) and (6) we present the results using 2SLS, which allows us to immediately estimate the average effect on car ownership, $\Delta E[C]$.[82] Our preferred estimate in column (6) indicates that the marginal effect of parking costs on car ownership is statistically significant at the 1% level and can be interpreted as an increase in parking costs of €100 is associated with a reduction in average car ownership of 0.017.[83]

[82]Results from a control function ordered logit model are essentially the same and are available upon request.

[83]In general, the control variables have plausible signs. Income, household size, age, level of education and distance to the nearest major train station have a positive effect on car ownership while building density and the availability of public transport in the near vicinity have a negative affect.

This is qualitatively the same as the outcome in column (4) using the MNL-CF approach and is larger than the estimate in column (2) where parking costs are not instrumented.[84] Given the result in column (7), this suggests that the implied price elasticity of car ownership is: $\varepsilon_P^C = -0.7$.[85] The results can also be interpreted in standard deviations (see column (3) of Table 3.B.6 in Appendix B). A one standard deviation increase in parking costs (€503) is associated with a reduction in average car ownership of 0.085.

The implied elasticity is within the range (but at the higher end) of estimates from the literature, which ranges between $[-0.3, -0.8]$.[86]. Car ownership elasticities with respect to fuel prices are generally below -0.3, so parking costs seem to have a stronger effect on car ownership than variable costs such as fuel prices (Goodwin et al., 2004; De Jong et al., 2009). This suggests that, at least for the Netherlands, permit pricing may be an effective tool to reduce car ownership. This likely reflects the availability of close substitutes to cars, such as public transport and bicycles, in the dense metropolitan regions we focus on.

It is important to put this result into context. Average car ownership in the city centres is around 0.90, while in the periphery it is 1.27, so the difference is 0.37 (see Table 3.B.7 in Appendix 1). Meanwhile parking costs in these areas are €1023 and €395, respectively, so the difference is around €630 (see Table 3.5). This suggests that, conditional on household and location characteristics, parking costs explain $-0.017 \times \frac{630}{100} = 0.11$ or around 30% of the difference in car ownership between the centre and periphery.[87] This seems realistic given the large parking costs, relative to ownership costs. The remaining difference in car ownership rates can likely to be explained by substitutes to cars available in the city centre such as walking and cycling, sorting of households and the difficulty of driving in the city centre, which are controlled for in our regressions.

Table 3.B.2 and 3.B.3 in Appendix B suggest that the most important control variables are household type and location characteristics such as distance to the nearest major train station, distance to the metropolitan city centre and building density.

[84] This suggests that reverse causality and measurement error indeed cause a bias towards zero.

[85] Annual average car ownership costs, excluding parking, are assumed to be €5000 (Nibud, 2017). See Appendix 2 for the full calculation. If $\Delta Pr[1]$ is zero, $\Delta E[C] = -0.017$ while if we include $\Delta Pr[1]$, $\Delta E[C] = -0.015$.

[86] Note the range presented is based on the elasticities with respect to purchase or fixed costs. See Dargay (2002); De Jong et al. (2009); De Groote et al. (2016); Seya et al. (2016).

[87] The magnitude is slightly higher for Amsterdam and The Hague at around 40%, while in Rotterdam and Utrecht it is around 15%.

Table 3.7: Sensitivity: Heterogeneous effects

	(1) Flats	(2) Houses	(3) Renter	(4) Amsterdam	(5) Rest
$\Delta E[C]$	-0.0130***	-0.0185**	-0.00734*	-0.0287	-0.0152***
	(0.00443)	(0.00829)	(0.00379)	(0.0186)	(0.00526)
ε_P^C	-0.66***	-0.71**	-0.54*	-1.53	-0.62***
	(0.22)	(0.32)	(0.28)	(0.99)	(0.22)
Controls (18)	Y	Y	Y	Y	Y
Year FE's (10)	Y	Y	Y	Y	Y
Mun FE's (45)	Y	Y	Y	Y	Y
Mean car ownership	0.99	1.30	0.68	0.94	1.22
KP F-statistic	33.60	9.30	35.24	3.76	16.86
# Parking-districts	484	480	492	49	445
# Households	29,222	65,700	52,871	7,517	91,142

Notes: Dependent variable is the number of cars per household. Standard errors are in parenthesis and are clustered at the parking-district level. 'Ams' refers only to the municipality of Amsterdam while 'Rest' refers to all other municipalities. All models are estimated using 2SLS. Kleibergen-Paap First stage F-statistic is presented. We directly estimate the change in average car ownership, $\Delta E[C]$, from a €100 increase in parking costs as the marginal effect of parking costs on car ownership. See Appendix 2 for calculation of ε_P^C, the implied price elasticity of car ownership, standard errors are calculated using the delta method. The elasticity is corrected for differences in mean car ownership between groups, as indicated above. See Table 3.B.3 for first stage results. Stars denote * p < 0.1, ** p < 0.05, *** p < 0.01.

3.4.3 Sensitivity

The results indicate that an increase in residential parking costs of €100 is associated with a reduction in average car ownership of around 0.017, indicating an implied price elasticity of car ownership of -0.7. In this section we perform a range of robustness checks.

In Table 3.7 we test the robustness of the specified demand for cars over various sub-groups.[88] In columns (1) and (2) we estimate specification (3.10) separately for households living in flats and single-family houses and find that the elasticities are the same. In the analysis above, we exclude renters because our estimated parking costs (using house prices) most likely reflect prices faced by owner-occupiers which may differ from parking prices faced by renters. In the Netherlands, the majority of urban renters live in public housing which generally does not have private parking. Therefore, these residents will mainly park on-street using a parking permit. If (public) renters respond less to cruising (time) costs, as they have lower incomes, the elasticity may be smaller. In column (3), we check whether renters have a different car

[88]Note the elasticity takes into account differences in mean car ownership between groups.

demand function as compared to owner-occupiers. The results suggest that renters may be slightly less responsive to changes in parking costs, however the elasticity is not statistically different.

In the municipality of Amsterdam, most districts allow a maximum of only one permit, while some districts also have waiting lists and permit fees are substantially higher than in other metropolitan areas. Therefore, households may be willing to pay more for a private spot and also may respond more strongly to permit fees. Therefore, in columns (4) and (5) we estimate the model separately for Amsterdam and all other municipalities. The effect of parking costs on average car ownership is roughly twice as high in Amsterdam. However, because it is imprecise, the estimate is statistically indistinguishable from the effect in other municipalities.

We also test the sensitivity of the results to various functional form assumptions and bandwidth sizes (see Table 3.B.5 in Appendix B). We specify the functional form of control variables more flexibly by measuring income, age, distance to the city centre and availability of public transport using more detailed categories.[89] The average change in car ownership increases slightly to -0.018. We also test whether changing the functional form of the instrument from linear to quadratic affects the results. Column (2) suggests that the marginal effect declines slightly to -0.014. In column (3) and (4) we test the sensitivity of the implied elasticity to alternative discount rate assumptions. Lower (higher) discount rates are associated with larger (smaller) elasticities, suggesting our estimate is conservative.

Lastly, we test the effect of adjusting the bandwidth used to estimate parking costs in step 1. Table 3.B.6 in the Appendix B indicates that a one standard deviation increase in parking costs has a similar effect on vehicle demand with lower bandwidths implying larger elasticities. We decide to take a conservative approach and use the implied elasticity for $h = 2$.

3.4.4 Counterfactual analysis

In order to apply our estimates, several assumptions are required. Most importantly, we assume a *partial equilibrium* setting where residence and job locations are fixed, therefore commuting patterns remain unchanged. We also assume that households respond to changes in (monetary and time) costs in the same manner, that vehicle

[89]Income is split into 6 categories: <20k, 20-40k, 40-60k, 60-80k, 80-100k and >100k. Age is split into 4 categories: <26, 26-45, 45-65 and >65. Distance to city centre is split into 10 1km bands and public transport availability is split into number of bus, metro and train stops within 100, 250 and 500 meters.

externalities are zero and that the implicit price for a *marginal* parking spot applies to all households. Additionally, we assume that cruising costs are zero in the periphery, where there is no paid parking. Therefore the value of a private off-street parking spot in the periphery captures the security value attributed to private parking, which we assume does not vary systematically over space. As such, we can calculate annual cruising costs per district, ζ_j, as the difference between the implicit parking cost, $\widehat{\rho_j}$, minus permit fees, F_j, and the parking cost in the periphery, $\widehat{\rho_p}$ or $\zeta_j = \widehat{\rho_j} - F_j - \widehat{\rho_p}$.

We follow De Groote et al. (2016) and assume a constant-elasticity inverse demand function: $D(Q) = P_c(Q/Q_c)^{\frac{1}{\varepsilon}}$, where P_c is the total current annual cost of owning a car, Q/Q_c is the average number of cars in the new scenario, Q, relative to the initial average number of cars, Q_c, and ε is the price elasticity of car ownership which is assumed to be constant over space. This functional form better accounts for the non-linearity in demand responses, which is important as we may want to consider large changes in parking costs.[90] We assume that the supply curve is fully elastic. Therefore the marginal cost of adding or removing a car is roughly constant and equal to the total average car costs excluding parking, which are around €5000, plus parking costs, $\widehat{\rho_j}$. This would imply a supply curve: $S(Q) = 5000 + \widehat{\rho_j}$. As we assume zero externalities, welfare effects in the car market can be calculated as the difference between the inverse supply and demand function.

Given these assumptions, we use information about current parking and vehicle markets to provide back-of-the-envelope calculations that approximate the effect of changes in parking costs (see Appendix 1 for calculations and an example). Estimates are based on a cross-section of transactions and households, therefore represent long run effects. We discuss the main implications of these assumptions in Section 3.4.5.

3.4.4.1 Implications for parking policy

Residential parking permits are offered at a fraction of the cost in the Netherlands. Currently, the highest annual permit fees in the country are in the centre of Amsterdam and cost €500 while the market value of a parking permit is around €3600 (Van Ommeren et al., 2011).[91] We apply our estimates to gain insights into the potential implications of raising residential permit fees in the city centre of Amsterdam

[90] Note assuming a linear demand function, whereby the change in car ownership equals $\Delta C = -0.017 \cdot \Delta P$ will likely overestimate the impact for large changes in P.

[91] Note, we estimate the implicit parking cost, conditional on the current number of permits which is likely to be an underestimate of the market value of a parking permit. Meanwhile, €3600 is likely to be an upper bound estimate of the market value as the average residents value for a permit is likely to be lower than a household with private parking.

to the market value estimated in Van Ommeren et al. (2011).

Increasing permit fees will likely raise overall parking costs in the short run, however, the effect on car ownership is likely to be smaller in the long run as higher costs induce households to give up their car which results in less cruising and shorter waiting lists.[92] We deal with this by considering two extreme cases, assuming; (A) that private cruising costs are unchanged and (B) that private cruising costs are zero when permit fees equal the market value.

In case (A), raising permit fees by €3100 (an increase in the total annual car ownership costs from around €6600 to €9700), is expected to reduce car ownership by approximately 24 percent and is associated with a welfare gain of around €300 per household (see Appendix 1 for calculation). In case (B), cruising costs, which account for around €800 in private time costs, fall to zero. This results in an increase in car ownership of around 5 percentage points, corresponding to a rebound effect of 20%. Therefore, the decline in car ownership is lower overall, 19 percent, and is associated with a lower annual welfare gain of around €245 per household.

3.4.4.2 Implications of automated vehicles

In the near future, automated vehicles (AVs) may not necessarily require parking which has implications for vehicle demand in cities. In a residential context, if households do not need parking anymore, there will likely be three types of welfare effects from: (1) not facing cruising costs, (2) increases in vehicle demand and (3) the value of re-purposing land currently designated to parking (Fagnant and Kockelman, 2015; Zakharenko, 2016). Our results allow us to provide estimates for (1) and (2).

We consider two scenarios for AVs. On the one hand, if households own private AVs and parking costs at the residence are sufficiently high, it is likely that AVs will be parked at locations in the periphery, where parking costs are relatively low. At these locations, parking costs will approximately equal the reservation value of land plus additional costs of traveling to and from the parking area (Zakharenko, 2016). Therefore in scenario (A), we assume parking costs approximately equal implicit parking costs in the periphery, $\rho_j^A = \widehat{\rho}_p$. On the other hand, if AVs are shared, then cars will only need to be parked during the evening and parking costs will be almost zero as they are shared between many users. Therefore, in scenario (B), we assume households incur zero parking costs, so $\widehat{\rho}_p = 0$ and $\rho_j^B = 0$.

[92]Note for simplicity we include all additional costs such as waiting times under the header cruising costs.

Table 3.8: Implications of AVs

Scenario A: Private AV					
	Amsterdam	Rotterdam	The Hague	Utrecht	Overall
Δ Car demand (%)					
Centre	16	4	8	4	8
Urban ring	9	8	3	0	5
Periphery	0	0	0	0	0
Δ Welfare (€/yr)					
Centre	641	256	623	235	445
Urban ring	484	632	187	0	321
Periphery	0	0	0	0	0
Scenario B: Shared AV					
	Amsterdam	Rotterdam	The Hague	Utrecht	Overall
Δ Car demand (%)					
Centre	22	11	13	11	14
Urban ring	14	15	7	6	11
Periphery	5	7	4	6	5
Δ Welfare (€/yr)					
Centre	962	714	985	690	832
Urban ring	843	1194	546	422	760
Periphery	450	629	415	610	515

Notes: In scenario (A), parking costs equal the implicit cost in the periphery. In scenario (B), parking costs are zero. As in Table 3.5, we define the 'centre' as < 2 km radius from the city centre, the 'urban ring' is between 2 and 5 km and the 'periphery' > 5 km. Δ Welfare represents the annual gain for an average owner-occupier household. See Table 3.B.7 in Appendix 1 for additional information.

To calculate (1), the welfare effect from not facing cruising costs, we compute the annual cruising costs per car, ζ_j, and transform this into an average welfare effect per household by multiplying ζ_j by the average number of cars per household, $\overline{C}_j$. Therefore, $\Delta W_{1j} = \zeta_j \cdot \overline{C}_j$. The welfare effect (2), from additional vehicle demand, is calculated as $\Delta W_{2j} = \int_{Q_c}^{Q'} (D(Q) - S(Q))dQ$, where Q' equals the average number of cars, given parking costs as specified in scenario (A) and (B).[93]

The counterfactual results are shown in Table 3.8. We focus on the "Overall" effects for an average owner-occupier household in the far right column. In scenario (A), AVs are privately owned and therefore parking costs equal the implicit cost in the periphery. This is expected to increase car demand by around 8 percent in the centre,

[93]We note that car ownership is *currently* a pre-requisite for car use. However, in the future, this is unlikely to be the case as AVs can be shared and used on demand. Therefore we consider our estimates for the effect of residential parking costs on car ownership (2) as providing an indication of the effect of parking costs on the extensive margin, i.e. whether households use a car.

5 percent in the urban ring and there is no change in the periphery. This is associated with annual gains per household of around €450 in the city centre, €300 in the urban ring and zero in the periphery.

In scenario (B), AVs are shared and therefore parking costs approach zero. As a result, car demand is predicted to increase by around 14 percent in the centre, 11 percent in the urban ring and 5 percent in the periphery. Annual gains per household are around €850 in the city centre, €750 in the urban ring and €500 in the periphery.

Overall, it appears that car use is likely to increase substantially if residents no longer face parking costs, with larger effects in denser urban areas where parking costs are high. Given that annual average travel distances per car are approximately 13,000km, additional vehicle demand may result in up to 1600km of additional annual car use by households in city centres.[94] The largest welfare gains arise from eliminating cruising costs, which are larger in areas with higher parking costs and higher car ownership. Meanwhile, the welfare gains in the car market, ΔW_{2j}, which are small, are likely to be lower due to vehicle externalities, such as congestion, pollution and injury, which are assumed to be zero in this application (Glaeser and Kohlhase, 2004; Sovacool, 2009).

In a realistic future scenario, one would expect that there will be both private and shared AVs, therefore the effects of reduced parking costs are likely to be somewhere in between the two cases presented.

3.4.5 Discussion

It is important to discuss the uncertainties from our application and implications of our assumptions as there may be reasons to believe that the effects could be over or under-estimated. Implicit prices from a hedonic model are an outcome of both supply and demand. Therefore, parking costs may be measured with error when considering a *large* change in parking demand. Additionally, estimated parking costs in the first step may be overestimated if off-street is preferred to on-street parking, conditional on search, or if parking policy is not binding. We are however not concerned with measurement error for our estimates of parking costs in the first step because we use an instrumental variables approach to estimate the elasticity in the second step. In our analysis, we focus on owner-occupiers which, as we show in the sensitivity analysis, may respond more strongly to parking costs than renters.

[94] Assuming that residential parking costs do not affect the number of kms travelled per vehicle and that new users utilise the car as intensively as an average current user.

It is more likely however that the estimated welfare changes for owner-occupiers are conservative. Firstly, the elasticity may vary over space. As there is a higher availability of substitutes in the city centre, the average elasticity may underestimate the effect in the dense urban areas which we focus on. Secondly, residential locations may change. Sorting of households with a high propensity to drive, such as high income families, into currently expensive parking districts may also result in larger changes in vehicle demand. Finally, we do not consider additional traffic congestion externalities associated with cruising and vehicle use. This would cause an underestimate for the welfare gains from eliminating cruising and raising permit fees while overestimating the (small) gains from additional vehicle demand in the case of AVs.

3.5 Conclusion

This paper provides an approach to estimate local residential parking costs and examines to what extent these costs affect vehicle demand, taking endogeneity issues into account. We apply the methodology to the four largest metropolitan regions of the Netherlands. The findings suggest that parking costs vary substantially over space. For example, in the city centre of Amsterdam, the annual implicit cost of an off-street, outside, parking spot is around €1600, which is over 20% of total average car costs and four times higher than in the periphery. Average car ownership for owner-occupier households in districts with one standard deviation (€503) higher annual parking costs decreases by around 0.085, corresponding to a price elasticity of car demand of about -0.7. The disparity in parking costs between the city centre and the periphery explain around 30% of the difference in average car ownership rates between these areas, providing an additional explanation for why car ownership is lower in dense urban areas.

We employ the estimates above to investigate the implications for parking policy. The municipality of Amsterdam is currently determined to reduce private car ownership and promote more sustainable modes of transportation in the city (Gemeente Amsterdam, 2018b). One tool at their disposal is permit fees. The results, applied to the city centre of Amsterdam, indicate that raising annual permit fees in the city centre to the market value, an increase from €500 to €3600, is expected to reduce average car ownership between 19 and 24 percent, depending on whether the rebound effect from eliminating cruising is taken into account.

These estimates can also be useful to gauge the potential implications of AVs as households will no longer require parking directly outside their residence. Our estimates provide long run approximations for the effect of fully AVs on cruising costs and vehicle demand considering different assumptions about changes in parking costs. The findings indicate that the average annual welfare gain per household from not incurring residential parking costs is between around €450 and €850 in the city centre, depending on whether AVs are privately owned or shared. This is associated with an increase in car demand in the city centre by 8 to 14 percent. These effects are smaller outside the central urban areas where parking costs are lower.

While this paper focuses on the effects of parking costs on car ownership, further research should consider the value of re-purposing on and off-street parking in cities as the land value is likely to be large. Furthermore, additional attention should be placed on estimating the effect of parking policy on cruising costs to get a better understanding of the rebound effect from policies aimed at raising parking fees. Finally, further research should consider how the elasticity of car ownership with respect to parking costs is related to the availability of substitutes for the private car.

Appendix 3.A Additional descriptives

Here we report some additional descriptive statistics of variables used in the analysis.

Table 3.A.1: Descriptive statistics: Transaction variables (cont.)

	Mean	Std. dev	Min	Max
Number of rooms	4.00	1.30	1	23
Number of floors	1.96	0.93	1	8
Garden	0.49	0.50	0	1
Terrace	0.09	0.29	0	1
Balcony	0.43	0.50	0	1
Basement	0.01	0.08	0	1
Good maintenance inside	0.20	0.40	0	1
Good maintenance outside	0.16	0.37	0	1
Central heating	0.91	0.29	0	1
Property is monument	0.01	0.08	0	1
New property	0.03	0.17	0	1
Terraced property	0.28	0.45	0	1
Detached property	0.02	0.13	0	1
Semi-detached property	0.05	0.21	0	1
Corner property	0.10	0.30	0	1
Distance to highwayramp (km)	2.06	1.44	0	9
Distance to station (km)	4.57	3.72	0	25
# Transactions	535,097			

Table 3.A.2: Descriptive statistics: Household variables (cont.)

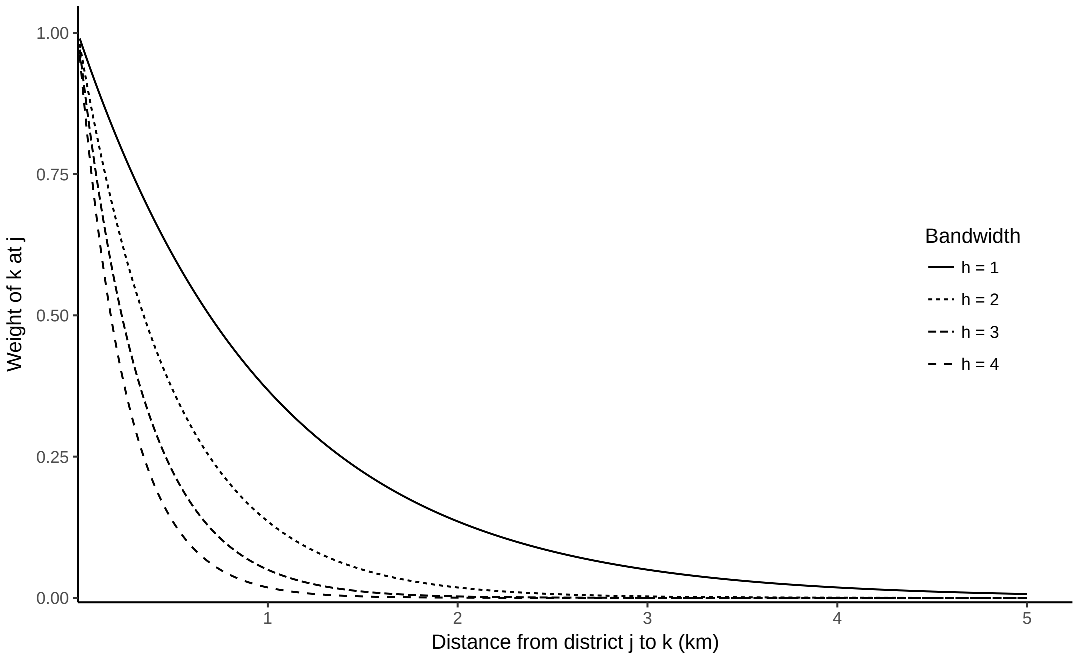
	Mean	Std. dev	Min	Max
Household single	0.12	0.33	0	1
Household couple	0.38	0.49	0	1
Household family	0.50	0.50	0	1
Distance to highwayramp (km)	2.69	1.97	0	10
Distance to station (km)	3.14	2.59	0	14
Distance to historic district (km)	2.75	2.36	0	13
Number of bus stops within 500m	5.82	3.82	0	55
Number of metros within 500m	0.24	0.58	0	4
Number of stations within 500m	0.06	0.23	0	1
# Households	98,659			

3.A.1 Other methodology

Here we report additional information on how the local weighted regression works in our application and provide an example of how the price elasticity of car demand is calculated.

3.A.1.1 Distance weighting function

Figure 3.A.1: Illustration of weighting using different exponential decay powers



3.A.1.2 Calculating the price elasticity of demand for car ownership

The elasticity of car ownership with respect to costs can be calculated as: $\varepsilon_P^C = \frac{\frac{\Delta C}{\overline{C}}}{\frac{\Delta P}{\overline{P}}}$, where ΔC represents the change in car ownership due to a change in parking costs, $\overline{C}$ represents average car ownership, ΔP represents the change in prices due to a change in parking costs and $\overline{P}$ is the average cost of owning a car, excluding parking. We assume the average annual total cost of car ownership, excluding parking costs,

$\overline{P}$, equals €5000 in the Netherlands (Nibud, 2017).[95] We also know that average car ownership is around $\overline{C} = 1.2$ in our sample. Furthermore, our preferred estimates suggest that a change in parking costs, $\widehat{\rho_j}$, of €100 is associated with a reduction in average car ownership of $\Delta C = -0.017$. Therefore $\varepsilon_P^C = \frac{-0.017}{1.2} / \frac{100}{5000} = -0.7$

[95]Vehicle costs for a new car range between around €3000 and €7500 per year, depending on vehicle size, and secondhand cars are around $25 - 30\%$ cheaper.

Appendix 3.B Additional results

Here we report additional results that were omitted from the tables presented in Section 5.4. This includes descriptives on estimated parking costs, control variables from our main results, first stage estimates and additional results from our sensitivity checks.

Table 3.B.1: Descriptives: Annual parking costs

Step 1	Mean	Std. dev	Min	Max
Parking cost (€/yr)	595.69	559.40	-1218	2820
Negative parking cost (%)	12.56	33.14	0	100
Negative (significant) parking cost (%)	4.73	21.22	0	100
Insignificant parking cost (%)	26.01	43.87	0	100
# Parking-districts	542			
Step 2	Mean	Std. dev	Min	Max
Parking cost (€/yr)	461.58	503.00	-1218	2820
Negative parking cost (%)	15.15	35.85	0	100
Negative (significant) parking cost (%)	6.14	24.01	0	100
Insignificant parking cost (%)	28.47	45.13	0	100
# Households	98,659			

Notes: Step 1 refers to the estimated parking costs weighted by the number of transactions in a parking district. Step 2 refers to the parking costs weighted by the number of households in a district.

Figure 3.B.1: Histogram of estimated parking costs

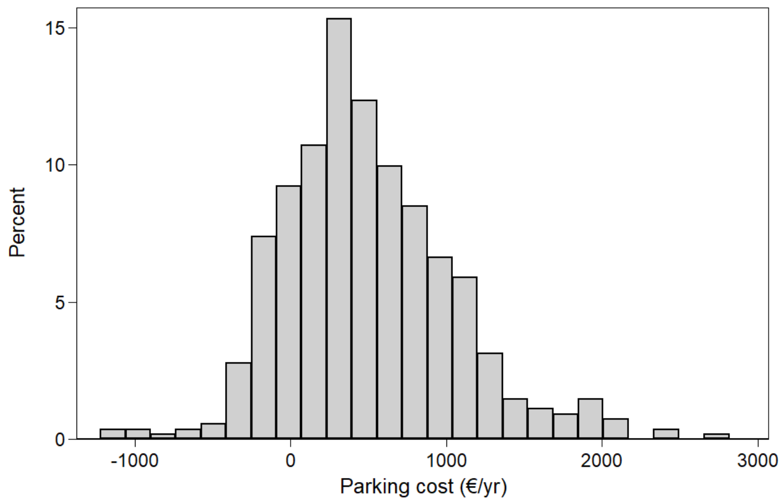


Table 3.B.2: Main results: Adding controls

	2SLS		
	(1)	(2)	(3)
Parking cost (€100/yr)	-0.0270***	-0.0166***	-0.0169***
	(0.00400)	(0.00487)	(0.00482)
Income (ln(€))	0.318***	0.315***	0.314***
	(0.00672)	(0.00627)	(0.00625)
Household size	0.0133***	0.0124***	0.0122***
	(0.00398)	(0.00392)	(0.00392)
Household couple	0.290***	0.289***	0.288***
	(0.00892)	(0.00869)	(0.00869)
Household family	0.378***	0.375***	0.374***
	(0.0132)	(0.0131)	(0.0131)
Age	0.0103***	0.0102***	0.0102***
	(0.000960)	(0.000939)	(0.000934)
Age^2	-0.000106***	-0.000108***	-0.000108***
	(0.00000944)	(0.00000922)	(0.00000918)
Education middle	0.0201***	0.0206***	0.0212***
	(0.00548)	(0.00545)	(0.00544)
Education high	0.00250	0.00277	0.00336
	(0.00733)	(0.00673)	(0.00670)
Distance city center (ln(km))		0.0211	0.0208
		(0.0201)	(0.0197)
Distance to highwayramp (ln(km))		-0.0186	-0.0188
		(0.0140)	(0.0139)
Distance to highway (ln(km))		-0.00329	-0.00202
		(0.00948)	(0.00957)
Distance to station (ln(km))		0.0292***	0.0326***
		(0.00806)	(0.00953)
Within historic district		0.0865***	0.0824***
		(0.0256)	(0.0252)
Distance to historic district (km)		0.0104**	0.0103**
		(0.00416)	(0.00419)
Building density (ln(m^2/ha))		-0.0473***	-0.0426***
		(0.00852)	(0.00837)
ε_P^C	-1.12***	-0.69***	-0.70***
	(0.17)	(0.20)	(0.20)
Controls (18)	Y	Y	Y
Year FE's (10)	Y	Y	Y
Mun FE's (45)	Y	Y	Y
First stage F-statistic	65.26	22.11	22.00
# Parking-districts	493	493	493
# Households	98,659	98,659	98,659

Notes: Dependent variable is the number of cars per household. Number of buses, metros, and train stations within 500 m omitted due to space constraints. Kleibergen-Paap First stage F-statistic is presented. Standard errors are in parenthesis and clustered at the parking-district level. The average change in car ownership, $\Delta E[C]$, is equal to the coefficient on Parking cost (€100/yr). Stars denote * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 3.B.3: Main results: First stage estimates

	MNL-CF		2SLS	
	(1)	(2)	(3)	(4)
Median construction year	-0.0927***	-0.0611***	-0.0927***	-0.0611***
	(0.0129)	(0.0154)	(0.0114)	(0.0130)
Income (ln(€))		0.0853		0.0853
		(0.0748)		(0.0754)
Household size		0.0378		0.0378
		(0.0380)		(0.0374)
Household couple		-0.156**		-0.156**
		(0.0758)		(0.0743)
Household family		-0.225*		-0.225*
		(0.120)		(0.119)
Age		-0.0130		-0.0130
		(0.00934)		(0.00934)
Age^2		0.000132		0.000132
		(0.0000985)		(0.0000996)
Education middle		-0.0680		-0.0680
		(0.0577)		(0.0585)
Education high		0.0866		0.0866
		(0.108)		(0.101)
Distance city center (ln(km))		-2.487***		-2.487***
		(0.878)		(0.746)
Distance to highwayramp (ln(km))		0.142		0.142
		(0.610)		(0.605)
Distance to highway (ln(km))		-0.0686		-0.0686
		(0.477)		(0.478)
Distance to station (ln(km))		0.293		0.293
		(0.443)		(0.417)
Within historic district		1.498*		1.498*
		(0.847)		(0.879)
Distance to historic district (km)		0.130		0.130
		(0.238)		(0.206)
Building density (ln(m^2/ha))		0.135		0.135
		(0.467)		(0.416)
Controls (18)	N	Y	N	Y
Year FE's (10)	Y	Y	Y	Y
Mun FE's (45)	Y	Y	Y	Y
First stage F-statistic	51.55	15.64	65.68	22.00
# Parking-districts	493	493	493	493
# Households	98,659	98,659	98,659	98,659

Notes: Dependent variable is the annual parking cost (€100/yr). Number of buses, metros, and train stations within 500 m omitted due to space constraints. Kleibergen-Paap First stage F-statistic is presented. Standard errors are in parenthesis and clustered at the parking-district level (and are bootstrapped in the MNL-CF model). Stars denote * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.B.4: Sensitivity: First stage estimates

	(1) Flats	(2) Houses	(3) Renter	(4) Amsterdam	(5) Rest
Median construction year	-0.0691***	-0.0528***	-0.0657***	-0.0575*	-0.0589***
	(0.0119)	(0.0173)	(0.0111)	(0.0297)	(0.0143)
Controls (18)	Y	Y	Y	Y	Y
Year FE's (10)	Y	Y	Y	Y	Y
Mun FE's (45)	Y	Y	Y	Y	Y
First stage F-statistic	33.60	9.30	35.24	3.76	16.86
# Parking-districts	484	480	492	49	445
# Households	29,222	65,700	52,871	7,517	91,142

Notes: Dependent variable is the annual parking cost (€100/yr). Kleibergen-Paap First stage F-statistic is presented. Standard errors are in parenthesis and are clustered at the parking-district level. Stars denote * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.B.5: Sensitivity: Functional form and discount rate

	(1) Flex	(2) Flexiv	(3) $\delta = 0.03$	(4) $\delta = 0.07$
$\Delta E[C]$	-0.0179***	-0.0144***	-0.0281***	-0.00843***
	(0.00556)	(0.00378)	(0.00803)	(0.00241)
ε_P^C	-0.75***	-0.60***	-1.17***	-0.35***
	(0.23)	(0.16)	(0.33)	(0.10)
Controls (18)	Y	Y	Y	Y
Year FE's (10)	Y	Y	Y	Y
Mun FE's (45)	Y	Y	Y	Y
First stage F-statistic	16.23	14.72	22.00	22.00
# Parking-districts	493	493	493	493
# Households	98,659	98,659	98,659	98,659

Notes: Dependent variable is the number of cars per household. Standard errors are in parenthesis and are clustered at the parking-district level. All models are estimated using 2SLS. Kleibergen-Paap First stage F-statistic is presented. The change in average car ownership, $\Delta E[C]$, from a €100 increase in parking costs is estimated directly as the marginal effect of parking costs on car ownership. See Appendix 2 for calculation of ε_P^C, the implied price elasticity of car ownership, standard errors are calculated using the delta method. Stars denote * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.B.6: Sensitivity: Bandwidth

	(1)	(2)	(3)	(4)	(5)
	$h = 1$	$h = 1.5$	$h = 2$	$h = 3$	$h = 4$
Parking cost (std)	-0.0819***	-0.0815***	-0.0848***	-0.0932***	-0.113***
	(0.0221)	(0.0225)	(0.0242)	(0.0293)	(0.0403)
$\Delta E[C]$	-0.0240***	-0.0188***	-0.0169***	-0.0159***	-0.0172***
	(0.00645)	(0.00520)	(0.00482)	(0.00499)	(0.00614)
ε_P^C	-1.00***	-0.79***	-0.70***	-0.66***	-0.72***
	(0.27)	(0.22)	(0.20)	(0.21)	(0.26)
Controls (18)	Y	Y	Y	Y	Y
Year FE's (10)	Y	Y	Y	Y	Y
Mun FE's (45)	Y	Y	Y	Y	Y
First stage F-statistic	30.96	25.74	22.00	16.18	10.85
# Parking-districts	493	493	493	493	493
# Households	98,659	98,659	98,659	98,659	98,659

Notes: Dependent variable is the number of cars per household. Parking costs are standardised, so the marginal effects represent a one standard deviation increase. Standard errors are in parenthesis and are clustered at the parking-district level. All models are estimated using 2SLS. Kleibergen-Paap First stage F-statistic is presented. The change in average car ownership, $\Delta E[C]$, from a €100 increase in parking costs is estimated directly as the marginal effect of parking costs on car ownership. See Appendix 2 for calculation of ε_P^C, the implied price elasticity of car ownership, standard errors are calculated using the delta method. Stars denote * p < 0.1, ** p < 0.05, *** p < 0.01.

3.B.1 Counterfactuals

We provide additional calculations for the welfare effects presented in Section 3.4.4. Given the constant-elasticity inverse demand function:

$$D(Q) = P' = P_c(Q/Q_c)^{\frac{1}{\varepsilon}},$$

we can calculate the new level of car ownership, Q' as:

$$Q' = \frac{P'^{\varepsilon} Q_c}{P_c^{\varepsilon}}.$$

In the counterfactual scenario where parking policy changes (Section 3.4.4.1), the new price, $P' = 9708$, the current price, $P_c = 6608$, and the current average car ownership level in the city centre is $Q_c = 0.750$. Therefore $Q' = 0.573$. Using these quantities and prices we can calculate the welfare effects in the car market as:

$$\Delta W = \int_{Q'}^{Q_c} (S(Q) - D(Q))dQ = \int_{Q'}^{Q_c} P'dQ - \int_{Q'}^{Q_c} P_c \left(\frac{Q}{Q_c}\right)^{\frac{1}{\varepsilon}} dQ.$$

Plugging in the prices and quantities indicates a welfare gain (equivalent to the deadweight loss) of around €300:

$$\Delta W = \int_{0.573}^{0.750} 9708dQ - \int_{0.573}^{0.750} 6608 \left(\frac{Q}{0.75}\right)^{\frac{1}{-0.7}} dQ \approx 1718 - 1414 \approx 304.$$

In Section 3.4.4.2, there is a downward shift of the supply curve as parking costs decline and therefore the change in welfare in the car market is calculated as: $\Delta W = \int_{Q_c}^{Q'} (D(Q) - S(Q))dQ$.

Table 3.B.7: Implications of AVs (cont.)

	Amsterdam	Rotterdam	The Hague	Utrecht	Overall
Car ownership, $\overline{C}$					
Centre	0.75	0.89	1.01	0.93	0.90
Urban ring	0.90	1.05	1.06	1.05	1.03
Periphery	1.24	1.28	1.26	1.30	1.27
Scenario A: Private AV					
Δ Parking cost (€/yr)					
Centre	1255	350	631	316	638
Urban ring	706	646	213	30	399
Periphery	0	0	0	0	0
Δ Car demand, C					
Centre	0.12	0.04	0.08	0.04	0.07
Urban ring	0.08	0.09	0.03	0.00	0.05
Periphery	0.00	0.00	0.00	0.00	0.00
Δ Car demand (km)					
Centre	1549	513	1071	486	952
Urban ring	1060	1108	384	53	685
Periphery	0	0	0	0	0
ΔW_1 (€/yr)					
Centre	566	249	597	229	421
Urban ring	455	605	183	0	310
Periphery	0	0	0	0	0
ΔW_2 (€/yr)					
Centre	75	7	26	6	23
Urban ring	29	28	3	0	11
Periphery	0	0	0	0	0
Scenario B: Shared AV					
Δ Parking cost (€/yr)					
Centre	1609	826	953	771	1023
Urban ring	1060	1122	535	420	792
Periphery	354	476	322	455	395
Δ Car demand, C					
Centre	0.16	0.10	0.13	0.10	0.13
Urban ring	0.13	0.16	0.08	0.06	0.11
Periphery	0.06	0.08	0.06	0.08	0.07
Δ Car demand (km)					
Centre	2103	1307	1705	1277	1628
Urban ring	1686	2078	1016	793	1452
Periphery	791	1094	731	1062	903
ΔW_1 (€/yr)					
Centre	832	673	922	652	768
Urban ring	774	1105	525	410	715
Periphery	439	609	406	592	502
ΔW_2 (€/yr)					
Centre	130	42	63	38	64
Urban ring	69	90	21	13	44
Periphery	11	20	9	19	14

Notes: All units are household averages except parking costs, which are per car. As in Table 3.5, we define the 'centre' as <2 km radius from the city centre, the 'urban ring' is between 2 and 5 km and the 'periphery' >5 km.

4

Smartphone usage and road safety

4.1 Introduction

Traffic accidents are an important loss to society. In the European Union (EU), for example, about 25,000 road users lost their lives due to traffic accidents in 2018. For every death on European roads, there are an additional 50 injuries of which 8 are severe and 4 cause permanent disability (European Commission, 2019b). Next to this physical harm, accidents also cause psychological suffering to those directly involved and to friends and relatives of the victims. Traffic accidents also lead to monetary losses due to damages to private and public property and are a major cause of traffic congestion. The total costs of traffic accidents in the EU are estimated to be about €280 billion, or 2% of GDP, which makes it the most important external cost of transportation (European Commission, 2019a). Similar numbers can be found for the United States and other countries (Blincoe et al., 2015).

These high costs explain the vast body of scientific literature on traffic accidents that

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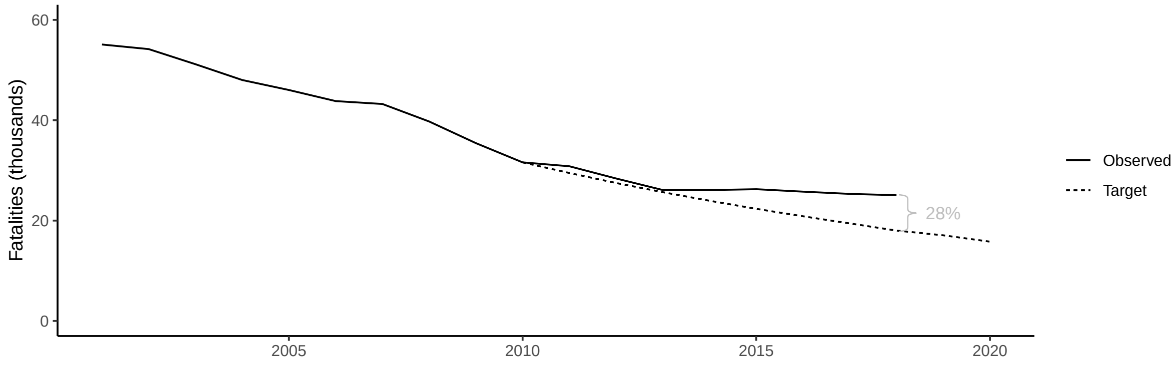


Figure 4.1: Road fatalities in the EU and 2020 policy target (European Commission, 2019b)

exists today, including important contributions from the field of economics on related topics such as the risk of drunk driving (Levitt and Porter, 2001a), the size of the accident externality caused by one typical additional driver (Edlin and Karaca-Mandic, 2006), and the effect of mandatory seatbelt laws on traffic fatalities (Cohen and Einav, 2003).[96] The substantial costs of accidents also provide governments with a strong rationale to prioritise safety in road design, and in traffic and vehicle-related regulation. Safety concerns in this respect largely shape policy decisions on aspects such as speed limits, road geometry, obligatory usage of seatbelts, and factors that affect the ability of road users to maintain attention on the driving task. This includes prohibiting the use of alcohol and cell phones by drivers. Figure 4.1 indicates that stricter safety regulations over the past two decades have had a promising impact on the number of road fatalities in the EU. However, progress in terms of reductions in road fatalities, as compared to the EU policy target formulated by the European Commission, began to diverge and stagnate in 2013, even after accounting for vehicle kilometres travelled.[97]

Despite regulations that forbid car drivers from using mobile phones while driving, effective regulation has proved to be difficult, and technological progress in recent years has transformed cell phones into omnipresent devices that can be seen as a major cause of distraction in traffic. Smartphones stand out as a major culprit, as they have enabled various novel distractions, including sending and receiving messages via numerous applications, news updates, video calling, and receiving notifications from social media platforms. In experimental settings, this has been shown to cause

[96]Other notable contributions include: Levitt and Porter (2001b), Adams and Cotti (2008), Jacobsen (2011), and DeAngelo and Hansen (2014).

[97]Data on vehicle kilometres travelled for all EU countries does not span back until 2000, so we plot fatality rates per million passenger-km for four major EU countries in Figure 4.A.1 of Appendix A, which shows a similar trend.

visual, cognitive, and physical distractions which result in longer reaction times, less awareness, and various other deficiencies which restrict full control of the vehicle (Zhao et al., 2013, Young et al., 2014, Haque and Washington, 2015).

Findings from the lab are generally corroborated by observational studies in naturalistic settings and crash-based studies (see e.g. Dingus et al., 2016, Redelmeier and Tibshirani, 1997, and McEvoy et al., 2005). However, various studies using field data fail to conclusively prove this relation.[98] In the first large scale field study of its kind, Bhargava and Pathania (2013) estimate the effect of mobile calls on accidents using a discontinuity in the price scheme at 9 pm between 2002 and 2005. They find a 7.2 percent increase in call likelihood after the price drop but no corresponding increase in the number of accidents at the 9 pm threshold. Further research on the effect of statewide mobile phone bans in the US indicates that the effects are short-lived, if detectable at all (Abouk and Adams, 2013; Burger et al., 2014).

The most recent studies that focus on smartphones find more conclusive negative safety effects. Hersh et al. (2019) exploit temporal variation in 3G coverage in California between 2001 and 2013 to study the effect of gaining access to mobile data on vehicle accidents. After controlling for vehicle kilometres travelled and road segment fixed effects, the authors find that crash rates increase by 1.1 percentage points when roads receive 3G coverage. Furthermore, Faccio and McConnell (2020) find that locations with a lot of activity of Pokémon Go (a popular video game app on the smartphone at the time) faced more vehicle accidents after the introduction of the game, suggesting that 136 of the total 2850 nation wide crashes (approximately 5%) in the five months after the introduction of the game could be attributed to it.

Although numerous studies have investigated the link between phone use and accidents, a substantial research gap prevails.[99] Most existing estimates are dated, while mobile phone use has dramatically changed since the turn of the century in terms of adoption, exposure and capabilities.[100] For example, in the much-cited study by Redelmeier and Tibshirani (1997), only 18% of drivers owned mobile phones which had limited capabilities, while in more recent studies, Bhargava and Pathania (2013) only focus on mobile calling and Hersh et al. (2019) end their study in 2013. Furthermore, studies that do address the interaction between modern smartphones, with data usage, and accidents, either focus on very specific non-generalisable phone-use (Pokémon Go in Faccio and McConnell, 2020), or only focus on highways (Hersh et al.,

[98] Drivers may also be able to navigate streets more easily using navigation applications, hence the effect of phone use on traffic accidents is not per se negative.

[99] See e.g. reviews by WHO (2011), Oviedo-Trespalacios et al., 2016, and Lipovac et al. (2017).

[100] Mobile phone subscriptions per capita have been above one in the world since 2016 (World Bank, 2019) and in 2018 smartphone penetration was above 70% in many developed nations (Newzoo, 2018).

2019). In addition, most studies do not account for unobserved factors that may be correlated to both phone use and accident likelihood, such as risk preferences at the individual level and demand factors at the aggregate level. Finally, as sample sizes were often small in experimental and crash-based studies, generalisation to aggregate effects is often problematic. Therefore, an important and ongoing research question is to what extent does smartphone use while driving affect the number and likelihood of traffic accidents.

We propose a novel approach based on field data and a natural experiment induced by a change in EU roaming regulations. The specific policy, imposed in June 2017, mandated mobile phone operators to abolish all roaming surcharges for EU customers travelling outside their home country network within the EU. The policy, dubbed *Roam Like at Home* (RLAH), implied that people travelling abroad within the EU now face their home fee, which is substantially lower than pre-policy charges. As a consequence, growth in roaming cellular traffic increased sharply after the policy. Mobile data use while roaming grew by over 200 percentage points, whereas local usage was not affected by the policy and faced stable growth rates.[101] We hypothesise that, as of June 2017, EU citizens driving abroad are more likely to be distracted by their phone, while nothing changed for local usage.

We use microdata on all police-reported road accidents in the Netherlands from 2014 until 2018. We then use vehicle registration information to classify which (foreign) drivers are plausibly treated by the RLAH policy. The causal effect of phone use on road accidents is then estimated using a difference-in-differences (DiD) approach, where we use the RLAH policy as treatment, and local users as control group. This allows us to overcome endogeneity issues from earlier studies due to measurement error in phone use and omitted variables. Our key identification assumption is that in the absence of the policy, the number of vehicle accidents by roaming users should follow similar trends to local drivers, for which we provide evidence in our parallel trends plot.

Our findings imply that the increase in phone use due to the policy causes the number of accidents to increase by around 10%. Under plausible assumptions, this implies a crash risk odds ratio of around 3.8. Under the assumption that this mechanism also carries over to local drivers and holds for other EU countries, our results then imply that each year as many as 2,500 road fatalities in the EU can be attributed to phone use. This suggests that about one-third of the gap between the EU target and the observed number of fatalities shown in Figure 4.1 could be reduced by successfully banning mobile phone use of drivers.

[101] Growth rates have been calculated using information from the International Roaming BEREC Benchmark Data Reports (for roaming) and the Dutch Authority for Consumers and Markets (for locals).

This study contributes to the existing literature in five ways. First, our results provide a causal estimate of phone use on road safety based on a novel method. Second, because our identifying variation comes from a very recent policy intervention, our estimates take into account modern distractions of smartphones, and particularly changes in mobile data use. Third, because our analysis is based on revealed and non-experimental field data of all registered accidents in the Netherlands, we are able to estimate an aggregate effect. This is especially relevant given the urgency of road safety issues and the rapid growth in cellular traffic. Fourth, with our approach, we can estimate how smartphone distractions affect accidents for different severity levels and on different road types. We show that phone distractions increase accident risk predominantly on local urban roads, which highlights that studies focusing solely on highways underestimate the total effect. In addition, our results indicate that both light accidents, as well as fatal accidents, increase due to smartphone use. Fifth, we introduce an identification strategy that is directly applicable to *all* other countries in the European Union, allowing for convenient cross-validation of our results using data from other countries in future research.

The rest of this paper is structured as follows. Section 4.3 describes the policy context, Section 4.4 explains the methods employed, and Section 4.2 presents the data we use. Section 4.5 discusses our results, robustness checks, and implications. Finally, Section 4.6 concludes.

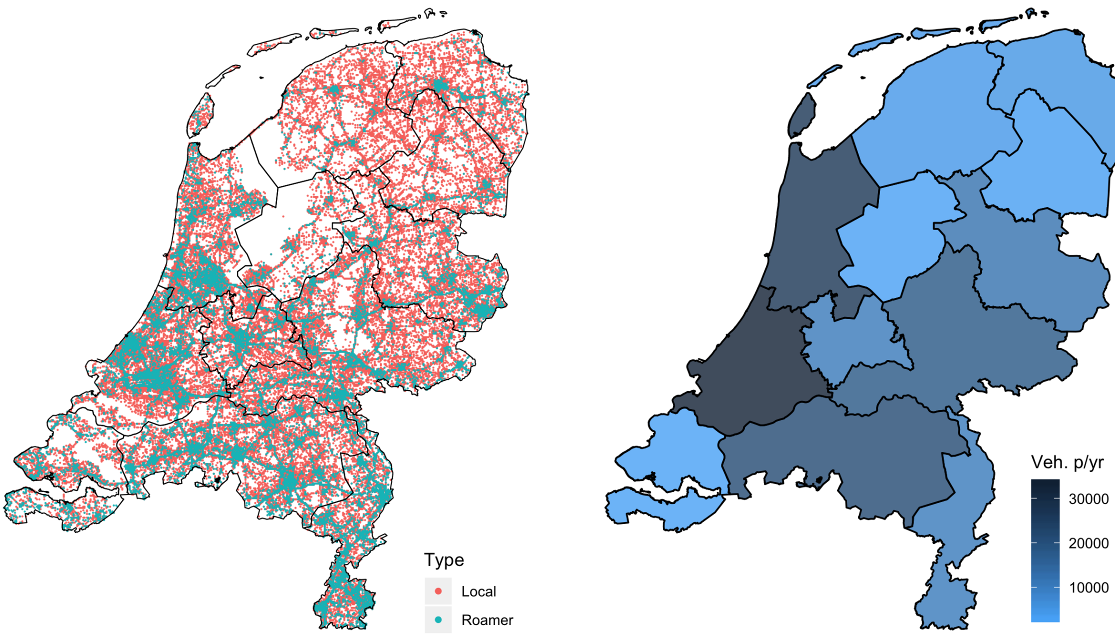
4.2 Data and context

4.2.1 Road safety data

We observe police reported accidents in the Netherlands as published by the Dutch Ministry of Infrastructure and Water Management (specifically 'Rijkswaterstaat'). The maps in Figure 4.2 plot the locations and annual counts of vehicles involved in accidents per province. The maps highlight that accidents are spread across the country, but more concentrated around urban areas and highways.

Our data contains characteristics of road accidents and of the parties involved.[102] For each accident, we observe accident circumstances, such as day of the week, time of the day, road type, weather conditions, and road surface conditions. Furthermore, the dataset contains vehicle related characteristics, such as vehicle type, vehicle ma-

[102] We use the full dataset available to researchers as we require privacy sensitive information on vehicle registration nationality. A publicly available version of the data is available on `data.overheid.nl`, but does not contain all party characteristics.



(a) Accident locations.

(b) Vehicles in accidents per year per province.

Figure 4.2: Maps of the Netherlands with accident locations and counts per province.

noeuvre just before the crash, sex and age of the driver, and the country in which the vehicle is registered.[103] Finally, party related variables are also reported and provide information such as age and sex of involved parties, casualty severity and whether the casualty was a driver, passenger, cyclist, or pedestrian.

We directly observe the vehicles' country of registration. Drivers of cars registered in EU countries, but outside of the Netherlands are likely to reside in those EU countries. Therefore, vehicle registration is a good proxy of whether the driver incurs roaming costs (before RLAH) or uses the local network instead.[104] To abstract from long term trends, we use data for the years 2014 until 2018, which contains 0.76 million vehicles involved in 0.44 million accidents. Most accidents have more than one

[103]For our particular application we cannot use most of these characteristics as they are often missing for non-local cars. This is because these data stem from the car registry in the Netherlands, which is not connected to databases from other countries. The data does not contain information on whether a car is rented or leased.

[104]Dutch law requires that any vehicle staying in the Netherlands for more than six months must obtain a Dutch licence plate. Note that, due to our difference-in-difference method, misclassification can pose a problem for the efficiency of our estimator, but will not bias our estimates under the plausible assumption that misclassification is not correlated to the roaming regulation.

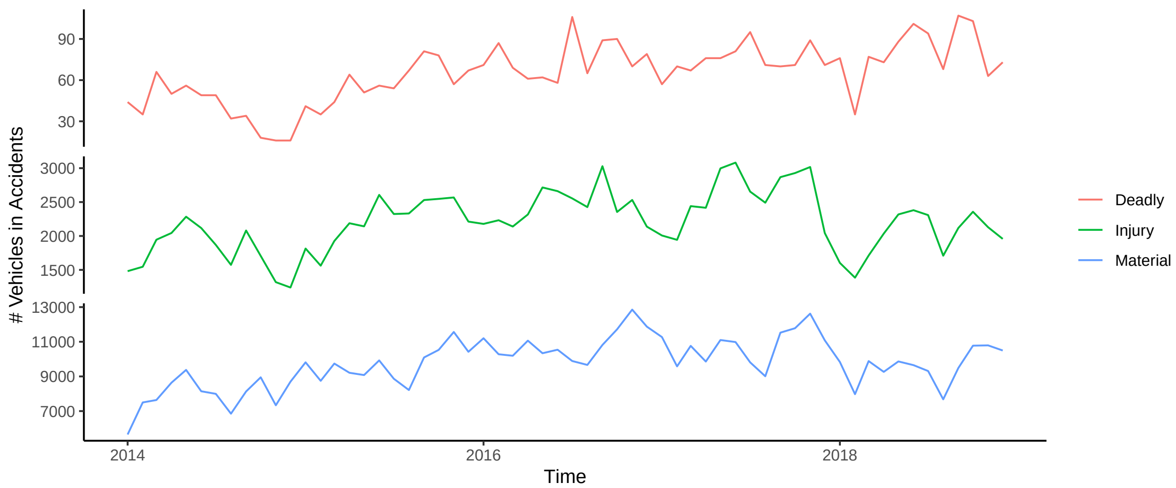


Figure 4.3: Number vehicles involved in accidents per month by severity.

vehicle involved (78%), therefore we use information at the party level to avoid measurement error which may be present at the accident level, as police reports do not indicate which party was at fault. We discuss this issue and how we deal with it in more detail in Section 4.4.2.

4.2.1.1 Trends in road safety

Figure 4.3 shows that there appears to be an increase in the number of vehicles involved in accidents over all levels of severity. Over the period of study, our data shows that the annual number of deadly accidents increased by around 20%, while the number of accidents involving injury and material damage increased by about 50%, with most of the change between 2014 and 2016. In an average month there are around 74 vehicles involved in deadly accidents, 2,381 vehicle accidents involving injury and 10,280 vehicle accidents involving material damage.[105]

[105]We also checked whether the number of vehicles per accident is stable over time, which turns out to be the case, both for accidents with only locals as well as accidents with at least one roaming user involved.

4.2.1.2 Grouping roaming drivers

We combine observations in our sample into six country groups for our main analysis. The aim of this grouping is to strike a balance between, on the one hand, optimally controlling for unobserved heterogeneity per country of origin (by means of group fixed effects), and on the other hand, preserving statistical power by avoiding zero counts (which are omitted due to the log transformation of the dependent variable, see Section 4.4 for a discussion).

The first group contains vehicles with a Dutch registration and is our control group (95.12% of sample). Second and third, are the two adjacent countries, with 1.76% of German vehicles and 1.04% of Belgian vehicles, respectively. The fourth group contains other western European countries, which account for 0.42% of vehicles in accidents. Drivers from these countries often visit the Netherlands as tourists.[106] The fifth group contains Romanian, Polish, and Bulgarian vehicles (1.32%) which are relatively common on Dutch roads due to joint economic activity and labour migration. More than for other cases, drivers from these labour migration countries may have a Dutch phone subscription and thus might not be treated by the RLAH policy. Therefore, it is important to include a separate fixed effect for vehicles from these countries. It also allows us to run a robustness check where we exclude vehicles from these countries, which highlights that vehicles from these countries do *not* drive our overall results (see Section 4.5.3.1). The sixth group contains all remaining EU countries (0.33%).

4.2.2 Descriptive statistics

4.2.2.1 Vehicles involved in accidents

Around 5% of vehicles involved in accidents are from roaming users, 46% of drivers are female and the average age is 42 years old. Of the total number of accidents, 0.58% are deadly, 18.7% result in injury, and 80.72% cause material damage only.[107]

Local and roaming drivers involved in accidents are roughly comparable, but roaming users tend to be younger, male, and drive more on fast roads than local drivers.[108] In terms of the damage reported, the share of material damage is relatively large for

[106] These are: France, Great-Britain, Denmark, Spain, Austria, Portugal, Luxembourg, Sweden, Italy, Ireland, Norway, and Finland.

[107] Table 4.A.1 in Appendix A presents the descriptive statistics for vehicles involved in accidents.

[108] Table 4.A.2 in Appendix A provides more detailed descriptives of vehicles involved in accidents by group.

Table 4.1: Descriptive statistics for province-month data

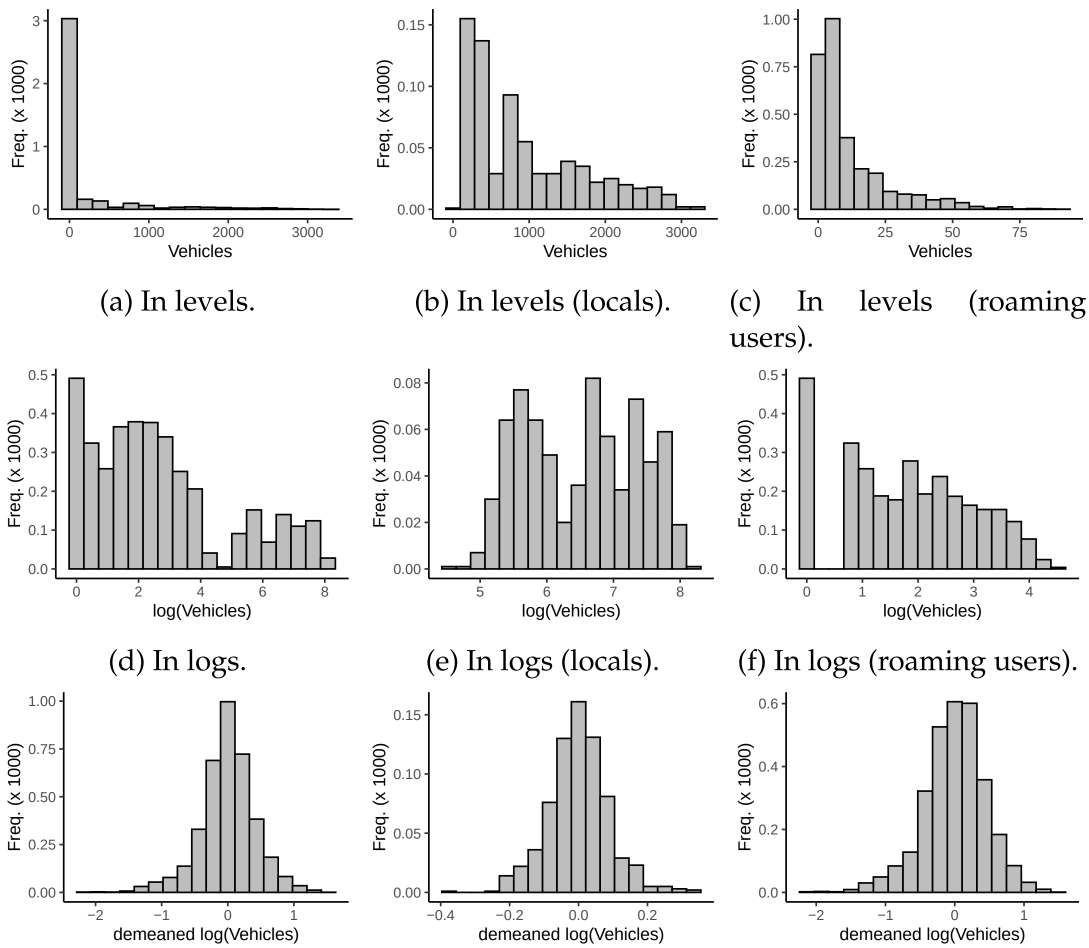
Statistic	N	Mean	St. Dev.	Min	Max
Panel A: Locals					
Vehicles in accidents	720	953.11	757.40	84	3,297
log(Vehicles in accidents)	720	6.52	0.86	4.43	8.10
No trucks	720	189.13	133.80	26	564
Single vehicle accidents (SV)	720	742.60	590.43	72	2,503
Hotel Nights ($\times$ 1000)	720	148.99	118.92	11	565
Panel B: Roamers					
Vehicles in accidents	3,600	9.78	13.41	0	92
log(Vehicles in accidents)	3,032	1.79	1.19	0.00	4.52
No trucks	3,600	1.68	2.53	0	20
Single vehicle accidents (SV)	3,600	6.16	9.54	0	71
Hotel Nights ($\times$ 1000)	3,600	22.21	72.31	0	707

roaming vehicles. This may be a reporting bias, as language barriers can make it more likely for the police to be called in these situations with only material damage, whereas locals may more easily settle without police present. Importantly, dissimilarities between local and roaming drivers do not threaten our identification under the plausible assumption that the RLAH policy does not induce sorting.[109] Dissimilarities become more relevant when generalizing estimated effects to the untreated population. We discuss the assumptions required to attribute the estimated effect to all drivers in Section 4.5.4.

4.2.2.2 Distribution of accidents

Our dependent variable is the number of vehicles involved in accidents, aggregated by province, month and country group. Table 4.1 presents descriptive statistics for various subsets. Naturally, the mean of the count of vehicles involved in accidents is in levels much larger for locals than for roaming users. In logs, however, the figures are more comparable and the standard deviation is in the same ballpark. Further we find that after controlling for the different mean levels —as we do by including

[109] Figure 4.A.3 in Appendix A shows that the age distribution of roaming users does not change considerably after the policy was implemented. We note, however, that even if we find a policy-induced sorting in the distribution of drivers in accidents, this does not necessarily bias our estimates, as it may be a result of the policy e.g. younger drivers may be more likely to use their phone and therefore be more represented in accidents, while the distribution of age groups in kilometres travelled may be the same.



(a) In levels. (b) In levels (locals). (c) In levels (roaming users).

(d) In logs. (e) In logs (locals). (f) In logs (roaming users).

(g) In logs demeaned. (h) In logs demeaned (locals). (i) In logs demeaned (roaming users).

Figure 4.4: Histograms of vehicles per month per province.

country fixed effects—the treated and control appear to have similar distributions (discussed below).

Figure 4.4 shows histograms of the dependent variable after log transformation and after demeaning for fixed effects. Panels (a)–(c) indicate that these empirical distributions are left-skewed, as to be expected from count data. Similarly, panels (d)–(f) show that after taking logs of these counts, distributions still seem to be slightly skewed to the left. However, if we demean by our panel and time fixed effects, as in panels (g)–(i), distributions seem quite symmetric, albeit with a larger variance for roaming compared to local users. This is non-problematic, however, when using standard errors that are robust to heteroskedasticity.

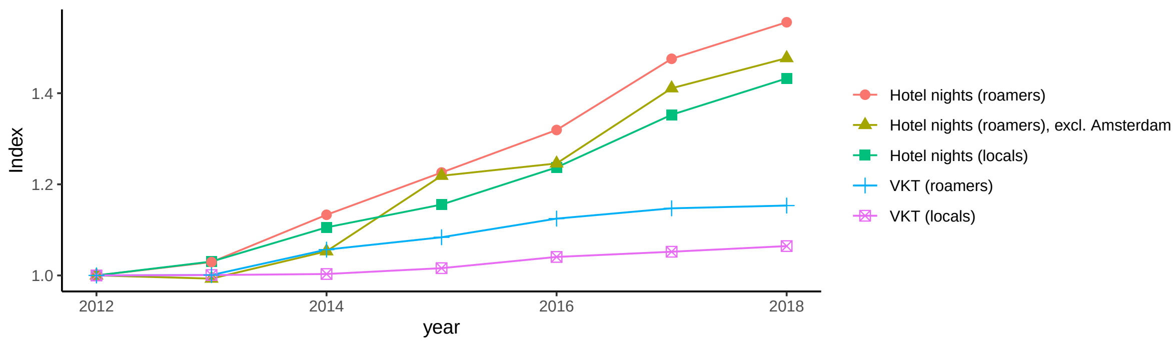


Figure 4.5: Trends in hotel nights and vehicle kilometers

4.2.2.3 Hotel nights data as proxy for traffic intensity

An important concern with our approach may be that country specific trends in traffic intensity, or vehicle kilometres travelled (VKT), might drive our results. For example, an increase in tourism over time may result in relatively more VKT by roaming users and therefore increase the likelihood of a roaming accident after the introduction of RLAH. We do not observe VKT for each drivers' country at the required level of temporal (monthly) and spatial (province) disaggregation. Instead, we use overnight stays in hotels, obtained from Statistics Netherlands (2019a), to proxy for changes in tourism and thereby monthly traffic intensity. For each province, we observe the number of overnight stays per month, disaggregated into guests' country of origin. We assess the quality of this proxy in two ways.

First, we observe country wide VKT at the annual level for locals and non-locals. Figure 4.5 shows annual growth rates of hotel nights and VKT for local and roaming (non-local) drivers. The figure highlights that over the course of the five years prior to the treatment, VKT by roaming drivers grew more compared to local VKT. However, a similar, yet even stronger trend is visible for hotel nights. Even when we exclude the province containing Amsterdam, an obvious hot spot of growth in hotel nights, we see a similar pattern. This suggests that we can capture trends in VKT with hotel nights, albeit potentially overestimating changes in VKT as it increases relatively less.

Second, we analyse how traffic intensity and the number of vehicles involved in accidents are related to hotel nights for Dutch drivers, for which we observe traffic intensities on highways at the province-month level (Statistics Netherlands, 2019b). Table 4.A.5 in Appendix A shows that, after controlling for time and panel (in this case simply province) fixed effects, there is no statistically significant effect of hotel nights for Dutch nationals with respect to traffic intensity, or number of vehicles involved in accidents. Importantly however, we do find a statistically significant and robust effect for the case of roaming drivers and the number of vehicles involved in accidents. This suggests that hotel nights are a good proxy for country specific changes in VKT from tourism and business related trips. Furthermore, the R^2 in column (2) is 0.99, which indicates that almost all of the variation in the traffic intensity can be explained by our fixed effects, suggesting that group specific changes in traffic intensity are unlikely to effect our estimates.[110]

4.3 The Roam Like at Home Policy

On 27 October 2015, the European Parliament adopted regulation No. 2015/2120 which prescribed that all roaming surcharges should be abolished within the EU.[111] Following a decade of EU roaming regulations which aimed to gradually reduce roaming fees within the EEA, the *Roam Like at Home* (RLAH) policy meant that, effective 15 June 2017, telecommunication network providers were required to abolish all roaming surcharges in addition to domestic retail prices for EU roaming customers.

The policy dramatically reduced the costs of phone use abroad, both compared to the gradual reductions prior to RLAH and compared to the pre-RLAH prices (BEREC, 2019). For example, leading mobile operators such as Vodafone Germany, offered daily roaming packages such as EasyTravel in early May of 2017 providing "phone calls, texting and surfing abroad [within the EU] just like at home" for a price of €2.99 per day. This equates to around €90 per month and is over four times more than standard domestic packages offering calls, texts and data at the time (Vodafone, 2017).[112] The special Eurobarameter (2018) survey, carried out one year after RLAH, suggests that awareness of RLAH was already high with 62% of Europeans that travelled in the previous 12 months being aware that roaming charges had been eliminated, and only 19% of travellers claiming to never use mobile data (down from 42%). Nevertheless, around 50% of the respondents still claim to restrictively use mobile data while abroad, suggesting that EU roaming users still use their mobile phones comparatively less than locals.

[110] Note that we find a borderline significant (significant only at the 10% level) negative estimate for hotel nights of locals in column (8). This might be an indication that drivers who are staying in a hotel, are driving more safely because they are unfamiliar with the area. This would be in line with findings in observational studies. Another possible explanation could come from region specific holidays that vary in timing between years for given regions, and between regions for given years.

[111] Roaming refers to mobile phones connecting to a cellular network abroad. In the absence of regulation, mobile network operators generally charge additional fees for using this service.

[112] Regulated wholesale data rates were capped at €0.05 per MB or €50 per GB, so using data outside of a data bundle may have been restrictively expensive.

To evaluate the effect of RLAH, we collect data on mobile phone usage of roaming users in the EU from the International Roaming BEREC Benchmark Data Reports and local usage from the Dutch Authority for Consumers and Markets (ACM).[113] Figure 4.6 plots the average monthly data traffic in MB's per roaming user for each quarter between 2012 and 2018, with the shaded region representing when RLAH was active.[114] It indicates that since RLAH was introduced, roaming usage appears to catch up with developments in local cellular data traffic. Roaming usage is still about four times lower than local usage after the policy, but this is a result of the relatively short period of time European tourists spend outside their country of residence (e.g. the average trip duration was about 8.4 nights in 2017 (Eurostat, 2019)). It also shows that cellular roaming traffic exhibits a strong upward growth trend for both groups and demonstrates a high degree of seasonal variation for roaming users. This is not surprising as technological advancements (e.g. introduction of 4G-network) and the increased adoption of smartphones has resulted in higher speeds, lower prices, and more demand, while tourism, and therefore roaming usage, tends to be seasonal. It is therefore useful, when comparing the annual growth rates of cellular traffic, to compare each quarter with the same quarter in the previous year.

Figure 4.7 illustrates that the RLAH policy resulted in a very large increase in the growth rate of phone use of roaming users one year after the policy while having *no discernible effect* on locals. Table 4.A.4 in Appendix A documents the average annual growth rate before and after the policy for roaming users as compared to locals. It indicates a substantial increase in the growth rate of roaming data usage by 200 percentage points, while texts and calls also increased by around 20 to 80 percentage points, relative to locals.[115] This further demonstrates that the policy had large effects on the overall phone use of roaming users, while especially effecting data usage.

[113] BEREC only includes information on the number of active roaming users, referred to as roaming subscribers in the BEREC reports, since the second quarter of 2016. BEREC considers a subscriber to be a roaming subscriber if roaming services were active *at least once* in the concerned period. In order to calculate the average monthly usage before this period, we predict the number of subscribers using a log-linear model with a time trend and quarter dummies. Using total data usage gives almost identical results (available from BEREC upon request). We document this in Appendix 1.

[114] Note that the second quarter of 2017 already contains 15 days during which the policy was active, namely the second half of June. Furthermore anticipating RLAH, several large network providers dropped roaming charges earlier in the year, such as Vodaphone UK in April (CNET, 2017).

[115] RLAH was not the only roaming policy introduced during the period of study. Other regulations, notably price caps, also resulted in moderate growth in roaming usage, which may explain the increased growth in data around the end of 2014 (BEREC, 2016).

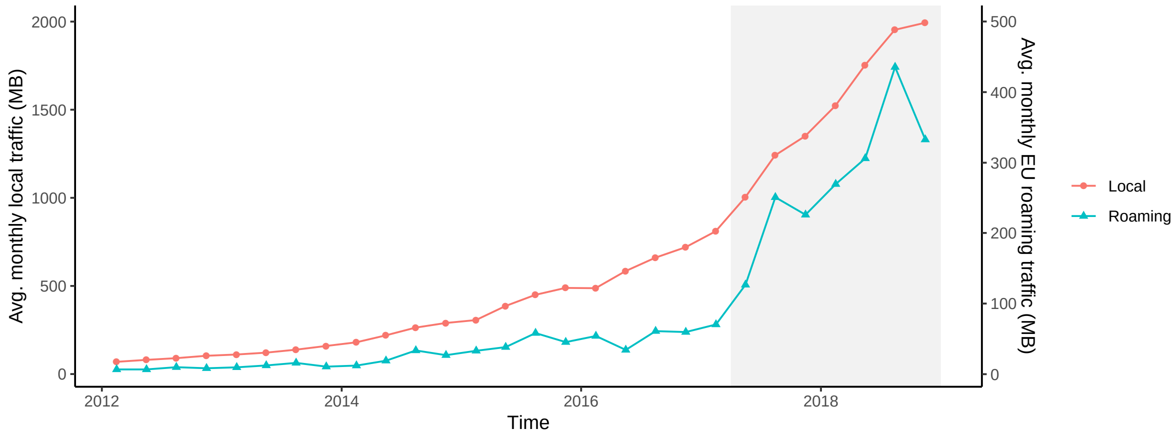


Figure 4.6: Average monthly data traffic per quarter.

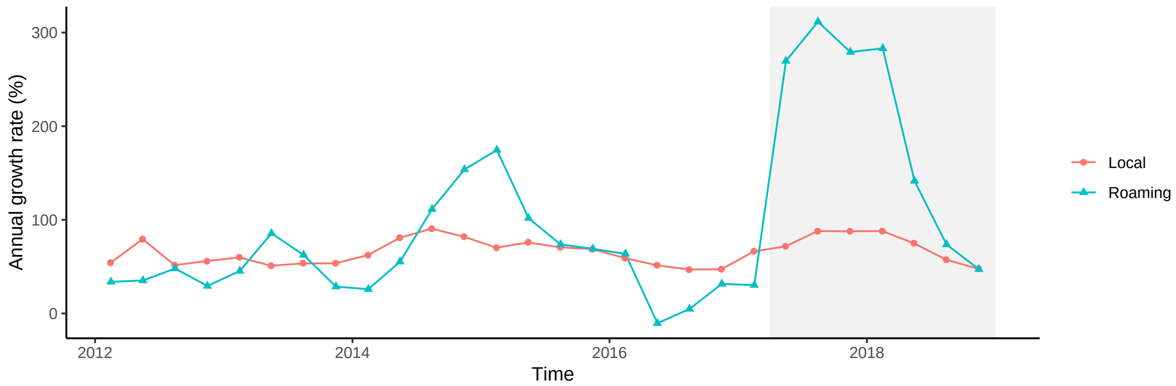


Figure 4.7: Annual growth rates in cellular data traffic per quarter.

4.4 Empirical methods

Our aim is to estimate how smartphone usage affects road safety. Because data on smartphone usage of drivers is privacy sensitive and not made available for research purposes, we use the implementation of the RLAH policy as a source of exogenous variation. We hypothesise that a substantial reduction in phone usage fees induces more phone use while driving, which in turn leads to an increase in accidents due to driver distraction. Unique for the RLAH price change, and essential for our identification strategy, is that fees for domestic phone use (i.e. within the home country) are not affected by the policy. This allows us to define a control group, in our case drivers with a Dutch phone subscription, and a treatment group, drivers with a phone subscription from any other EU country. As a consequence, we can employ a difference-in-differences (DiD) approach to estimate the effect of the policy-induced increase in phone use on road safety. Below, we first introduce the general statistical model and subsequently discuss how we deal with the statistical challenges that arise in our setting.

4.4.1 Statistical model

We use a standard DiD approach, where we estimate how the RLAH policy affects the number of vehicles involved in road accidents. We define V_{it} as the number of vehicles involved in accidents for each country-group in each province, indexed by i, at time t.[116] Countries are grouped to strike a balance between optimally controlling for unobserved heterogeneity per country of origin, and preserving statistical power by avoiding zero counts (see Section 4.2.1.2 for more details). We consider the general following model:

$$\log(V)_{it} = \beta T_{it} + \gamma \mathbf{H}_{it} + \delta \mathbf{W}_{it} + \phi_i + \kappa_t + \epsilon_{it}, \tag{4.1}$$

where $\log$ denotes the natural logarithm. The treatment effect, denoted by T_{it}, is a dummy equal to one after the policy was introduced for vehicles from roaming countries. We proxy for traffic intensity using vector $\mathbf{H}_{it}$, which contains separate control variables, in logs, for the number of hotel nights of locals and roaming users, and a dummy in case we observe a zero.[117] Further, vector $\mathbf{W}_{it}$ contains weather controls, that we include to improve the efficiency of the estimator.[118]

Finally, we include panel and time fixed effects. Time-invariant characteristics of drivers and the area in which they drive, such as the road network, attractiveness to tourists, and number of car users, are captured by a country-group province fixed effect, ϕ_i, which represents the panel element in our analysis. We also control for any unobserved time trends affecting all drivers, for instance due to road maintenance or infrastructure improvements, by including a time fixed effect, κ_t, for each year-month.

We note that using a smartphone was rather costly for roaming users before the pol-

[116]Because we essentially have a count model, our temporal and spatial resolutions are arbitrary. We aim for the most fine-grained resolution to maximally use variation over time and space. We are in this respect, constrained by the resolution of the essential control variables. We aggregate at the province-month level because this is the most fine-grained resolution for which we can control for country-specific VKT.

[117]We obtain hotel nights per province per country of origin from Statistics Netherlands (2019a), which is measured in thousands. In case of a zero, which we only observe for roaming countries, we set the value to one (so that the log is zero) and use a dummy to control for these cases separately. This means that we correct for any bias due to inflation at when a zero is reported.

[118]These include for each province and month the average temperature, average rainfall, and number of days with temperatures below 0 °C.

icy. It might therefore be useful to assume that before the policy roaming drivers did *not* use their phone at all while driving. However, if roaming users *did* use their phones while driving prior to new roaming regulations, we still accurately estimate the effect of the price drop, but underestimate the total effect of phone use. Our estimates should therefore be considered as a lower bound of the total effect of smartphone distractions on road accidents.

4.4.2 Measurement error

Measurement error poses a statistical challenge in our setting, because we do not directly observe within-vehicle phone use, nor the type of phone subscription drivers have. Below, we identify three implications of this challenge and discuss how we deal with them.

First, for multi-vehicle accidents, we cannot identify which driver caused the accident, if any at all. This means that we have measurement error in the dependent variable, which makes our estimates potentially imprecise, albeit still unbiased if the measurement error is random. We address this issue by focussing on vehicles rather than accidents because multi-vehicle accidents might include both treated and control-group drivers. In addition, we also perform a robustness check where we consider a subsample with single-vehicle accidents (e.g. a car crashing into a tree). This approach rules out measurement error of this sort but comes at the cost of having less statistical power, as only a small fraction of the accidents in the data are single-vehicle accidents (17.58%). As it is a priori not possible to decide which is the preferred approach, we report results for both estimation strategies.[119]

Second, some drivers of vehicles that are registered abroad might still have a Dutch phone subscription. For instance, drivers that live in bordering regions in Belgium or Germany and often work in the Netherlands. These drivers will be erroneously classified as treated, and will bias our estimates downwards.[120] To address this issue, we will run a robustness check where we exclude all border provinces, as it is likely

[119] Another related issue which is solved by taking single-vehicle accidents is that roaming accidents may result in more multi-vehicle accidents. This would violate the SUTVA, but it is unlikely to be problematic in this setting due to the size of the control group; around 95% of vehicles in our accidents sample are part of the control group. Also, we also checked whether the number of vehicles per accidents changes over time, which is not the case, both for accidents with only locals as well as accidents with at least one roaming user involved.

[120] Additionally, some roaming users might be driving a Dutch car, for instance, a rental car, and will hence be erroneously designated as untreated. This may lead to a small downward bias, however, due to a large number of accidents in the control group (local users) it is unlikely to have a substantial effect.

that this measurement problem is most pronounced in those regions.

Third, some roaming users may not have to pay smartphone charges themselves. One can think of unlimited subscriptions paid by drivers' employers or having a Dutch subscription while living just across the border. This insensitivity to the price would also result in a downward bias of the estimate. We address this concern in two ways. Firstly, we re-estimate our main model on a sub-sample where we exclude trucks and vans, assuming that drivers of these vehicles are most likely to have such arrangements with their employer, and secondly, on a sub-sample without bordering countries or typical labour migration countries.

4.4.3 Trends in vehicle kilometres travelled

A potential confounding factor is vehicle kilometres travelled (VKT) by roaming drivers. For instance, because countries and provinces vary in their popularity as a holiday destination over time (Taylor and Ortiz, 2009), there may be more roaming accidents due to increased tourism rather than due to increased phone distractions. Another potential reason for temporal variation in VKT by roaming drivers could come from changes in trade and business trips as a result of ongoing globalisation. Because these trends affect treated drivers (e.g. tourists) but not local drivers, it poses a potential threat to our identification strategy and may lead to overstating the effect of phone distraction on road safety.

Ideally, one would want to directly control for VKT to avoid any bias from traffic intensity, but this information is not available.[121] Instead, we show that the number of hotel nights per country of origin is a good proxy for both tourism and business-related traffic (see Section 4.2.2.3 for an extensive discussion on the quality of this proxy). This implies that, if the relation between traffic and hotel nights is stable over time, then controlling for hotel nights will absorb a bias that stems from VKT trends of roaming drivers.[122] Nevertheless, we also perform two additional robustness checks. Firstly, we include a roamer-specific linear time trend which captures nationwide trends in accidents of all roaming users combined. This approach then estimates the policy effect *conditional on* a roaming-user-specific trend in accidents, which provides a lower bound for the estimated effect. This time trend does, of course, also absorbs part of the treatment effect, such that this estimation is only useful to assess

[121] For non-Dutch vehicles, Statistics Netherlands only provides imputed annual figures of VKT for the whole country. For all traffic combined, there are intensity measures available at the province-month level. These will be used to validate our VKT proxy (hotel nights).

[122] This is a reasonable assumption for our five-year study period, but may not hold in the long run (e.g. if cheap flights and high-speed trains make cars a less attractive mode).

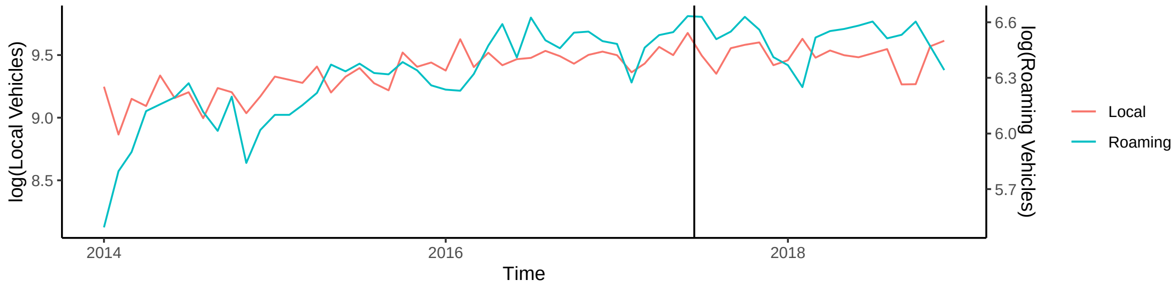


Figure 4.8: Roaming and local user vehicles in accidents.

Figure 4.9: Graphical representation of common trends in aggregated province-month data.

our estimates' sensitivity to trends. Secondly, we re-estimate our models using only two years of observations between July 2016 and July 2018 (i.e. one year before and one year after the policy), for which it is implausible that there are major trends in tourism transport modes conditional on hotel nights.

4.4.4 Standard errors

In our setting, the number of observations depends on an arbitrary temporal and spatial resolution. We aggregate vehicle data to province-month observations, to align the resolution with our control variables. However, if accidents are serially correlated, ordinary least squares (OLS) standard errors may be too small (Bertrand et al., 2004). To address this issue, we cluster our standard errors at the time-invariant level of a province and country-group, which leaves us with $12 \times 6 = 72$ clusters (12 provinces and 6 country groups). In addition, we run a robustness check where we ignore all time-series variation and aggregate our data into two periods, one before and one after the policy. This rules out any autocorrelation in error terms, and the outcome highlights that our results and standard errors are hardly affected by serial correlation.

4.5 Results

4.5.1 Parallel trends

We first examine overall trends of local vehicles (control group) and roaming vehicles (treated group) involved in accidents. Figure 4.8 shows that nationwide accident

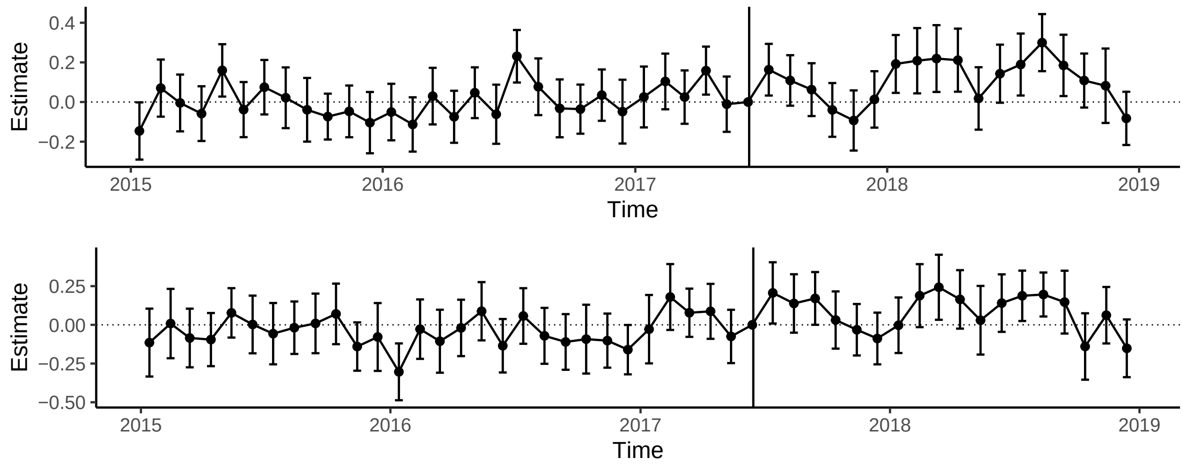


Figure 4.10: Treatment effect per month for full sample (top) and single-vehicles (bottom).

counts for these groups follow similar trends.[123] The figure also highlights that these measures are quite noisy and that no clear jump is observable around the policy introduction in 2017.

For a more rigorous analysis of a common trend, in Figure 4.10 we plot estimates of a monthly treatment effect, while including all controls and fixed effects as in our preferred specification in (4.1). Here, the coefficients are estimated using an indicator for whether the province-month count of vehicle accidents are for roaming users, interacted with year-month dummies.[124] The results in Figure 4.10 indicate that no clear pre-trend exists and that local drivers are a suitable control group for roaming drivers, conditional on controls and fixed effects. Furthermore, after the policy, there is a clear positive impact on accidents as indicated by the increased proportion of positive and statistically significant estimates.[125] This pattern is even more pronounced

[123] In January 2014 there are fewer roaming user accidents, this seems to be a reporting-issue in the data source. Our robustness check where we focus only on one year before and after the policy indicates that this issue does not affect our conclusions.

[124] Specifically, the figure plots the β_τ coefficients from estimating:

$$\log(V_{it}) = \sum_{\tau=-41}^{60} \beta_\tau R_{i,t-\tau} + \gamma \log(H_{it}) + \phi_i + \kappa_t + \epsilon_{it}, \tag{4.2}$$

where $R_{i,t-\tau}$ is an indicator variable for whether the vehicle count is for roaming users or not, interacted with a year-month dummy, and β_τ is the effect of the policy for each year-month t. To be able to include the seasonality fixed effect κ_t in this setting, we omit the treated×year-month dummies for the first full year; otherwise perfect multicollinearity emerges. The error bars represent robust 95% confidence intervals for each monthly point estimate.

[125] 44% of the coefficients are positive and statistically significant post-policy as compared to only 10%

Table 4.2: Main regression results

	log(# Vehicles in Accidents)				
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.124***	0.125***	0.176***	0.089***	0.094***
	(0.034)	(0.035)	(0.025)	(0.030)	(0.031)
Roamer × trend				0.003**	
				(0.001)	
log(Hotel nights roamers)					0.298***
					(0.076)
log(Hotel nights locals)					-0.089
					(0.063)
Temperature		0.057***	-0.005*	-0.005*	-0.004*
		(0.016)	(0.003)	(0.003)	(0.002)
Rain		0.061	-0.001	-0.001	0.003
		(0.061)	(0.012)	(0.012)	(0.013)
# Frost days		0.157***	0.025*	0.025*	0.019
		(0.040)	(0.013)	(0.013)	(0.012)
Time FE		Yes	Yes	Yes	Yes
Panel FE			Yes	Yes	Yes
Clusters	72	72	72	72	72
Local vehicles	686k	686k	686k	686k	686k
Roaming vehicles	35k	35k	35k	35k	35k
Observations	3,688	3,688	3,688	3,688	3,688
R^2	0.729	0.748	0.965	0.965	0.967

Notes: Column (1) is a basic DiD regression which includes a dummy for roaming user, policy and the interaction between roaming user and policy (denoted treatment effect). Robust standard errors in parentheses are clustered at the province and country-group level. Hotel nights are split into two orthogonal variables for local and roaming users. An additional dummy is included when hotel nights were inflated (only occurs for roaming users). ***, **, * indicate significance at 1%, 5%, and 10%.

in the bottom panel of the plot, where we focus specifically on single-vehicle accidents.

4.5.2 Estimation results

Table 4.2 shows the estimation results with incremental levels of controls and fixed effects. Column (1) shows that with only the minimal DiD controls, we find a statistically significant effect of over 12%.[126] Column (2) shows that overall time trends

pre-policy.

[126]Here we run the most simple DiD regression, which includes a dummy for the RLAH policy, a dummy for whether the country group consists of roaming users, and the treatment effect is the

(captured by year$\times$month fixed effects), and weather controls hardly change the estimated treatment effect. In column (3) we add panel fixed effects, where our panel identifier is a province-country group. This increases the point estimates and lowers the standard errors, indicating that these fixed effects improve the efficiency of the estimator and suggests that accident counts are heterogeneous across provinces and country-groups. Column (4) shows that the estimated treatment effect declines significantly when we add a linear roaming-specific monthly time trend. This is potentially a bad control that can also pick up part of the treatment effect, but the results here imply that any major nationwide trends in accidents of roaming users only partially affect the results.

Our preferred specification is the one used in column (5), in which we include controls for hotel nights as a proxy for traffic intensity. We find a point estimate of 0.094 with a standard error of 0.031. This implies that the policy-induced increase in phone use leads to an increase in the number of vehicles involved in accidents of 9.91%, with a 95% confidence interval of $3\% - 17\%$. The point estimate declines as compared to (3) and the hotel nights elasticity of roaming users has the expected sign. It indicates that a 1% increase in hotel nights for roaming users is associated with an increase of around 0.3% in the number of vehicles involved in accidents. The hotel nights effect is insignificant for locals, conditional on our set of fixed effects. This makes sense as traffic intensity for roaming users is likely to follow seasonal tourist trends while most local traffic is generated by work commutes and other daily activities. Importantly, fixed effects already absorb overall trends in VKT, heterogeneity across provinces, and heterogeneity across vehicle countries. Therefore, the statistical significance of the hotel nights elasticity, and the fact that the point estimate of the treatment effect is smaller when we include hotel nights, highlights that we indeed capture country-specific long term trends in VKT.

4.5.3 Robustness checks

In this section we perform a vast range of robustness checks. Tables with results are available in Appendix B.

4.5.3.1 Measurement error and endogeneity

One type of measurement error arises because we do not accurately observe which vehicle potentially caused the accident. Table 4.B.1 in Appendix B shows estimation

interaction between these two dummy variables.

results using different subsets of accident types and vehicle involvement. Columns (1–2) show that focusing on different types of accidents yields very similar results. Excluding trucks and focusing on single-vehicle accidents leads to very similar or only slightly stronger point estimates. Focusing on single-vehicle accidents may suggest we reduce measurement error slightly, but again the point estimates are not statistically different from the main estimate.

As discussed before, our analysis may suffer from measurement error in the treatment assignment, for instance by having a Dutch phone subscription while still driving a non-Dutch car or vice versa. It is likely that measurement error is most pronounced in bordering provinces and for drivers with a close connection to The Netherlands. This can either be due to proximity (like bordering regions or countries) or due to strong economic links (e.g. labour migration). If we exclude bordering countries, we find somewhat larger effects while if we remove bordering provinces or drivers from labour migration countries, we find only slightly smaller effects. Excluding border provinces also mitigates potential concerns that border provinces face more VKT due to the policy, e.g. if people are more likely to go shopping across the border because phone usage is cheaper. Such an endogenous response might induce sorting and thereby poses a threat to our identification strategy. These results indicate that our results do not suffer from a severe downward bias from measurement error.

4.5.3.2 Accounting for VKT trends

So far, we have assumed that country-of-origin specific trends in VKT are well-captured by our hotel nights proxy. Results from Section 4.2 suggest that this is a plausible assumption. Nevertheless, to further rule out any issue with long-term trends in non-local road traffic as a potential confounder, in columns (1–2) of Table 4.B.2, we restrict our sample to one year before and one year after the policy (i.e. from June 2016 to July 2018). This approach yields an estimate of 6.8% for all vehicles and 14% for single-vehicle accidents which are very comparable to our main results. This highlights that long term trends in VKT cannot explain the observed increase in vehicles involved in accidents.

4.5.3.3 Accounting for auto-correlation in error structure

In our main analysis, we use the number of vehicles involved in accidents per province per month as the observational unit. If there is strong serial correlation, then OLS standard errors may be incorrect, even when clustering at a time-invariant level as we do (Bertrand et al., 2004). To deal with this issue in the most conservative way, we re-estimate our main models on data aggregated to pre and post-policy averages.[127] Columns (3–4) in Table 4.B.2 show that the statistical significance is only slightly lower as compared to our main analysis (the t-statistic = 2.1 as compared to 3.1 in our preferred specification). This provides strong evidence that serial correlation does not pose a threat to our statistical inference.

4.5.3.4 Weighting

Our aim is to approximately recover the phone-use effect *per driver*, rather than at a province level. This suggests that we should use sample weights for VKT at the individual level.[128] Because these data are not available on the vehicle accident level, we test the robustness of our results to four weighting schemes that are closely related to VKT.[129] As regions differ in the total number of roaming drivers involved in accidents, this also allows assigning higher weights to provinces that tend to have relatively more roaming drivers and therefore may be more informative. Table 4.B.3 shows that our main results hardly change if we use weights based on 1) roaming accident numbers, 2) total accident numbers, 3) traffic intensity, and 4) hotel nights. This suggests that our fixed effects and log-level specification already sufficiently account for differences in VKT between regions.

4.5.3.5 Accounting for zero counts

In our main analyses, we use a log-linear specification, which performs well with a sufficient number of accidents. However, during some months, for some country-groups, we observe few or even zero vehicles in accidents (14.02% pre and 4.95% post policy). These cases are naturally excluded from our log-linear regressions. However, they might be less likely to occur after the policy due to policy-induced phone distractions. As a consequence, our estimations might suffer from a slight downward bias by excluding more zero counts before than after the RLAH policy introduction

[127] After aggregating, the data represents the log number of vehicles, hotel nights, and weather conditions, by country group and province, for an average month in the pre and post data.

[128] Note however that weighting might lead to erroneously small standard errors when there is clustering in the disturbances (Solon et al., 2015). Therefore, as the latter is likely to be the case in our setting, we are cautious with weights and report the more conservative estimates (without weighting) as main results.

[129] Note that for accident numbers we use the time-invariant pre-policy number of roaming and total accidents.

for treated vehicles. To test if such a bias exists, we re-estimate our main specification as in (4.1) using a Poisson pseudo-maximum likelihood count model. Table 4.B.4 presents the results from this re-estimation, which allows us to include all province-month observations.[130] The coefficients are remarkably similar and in column (5), our preferred specification with hotel nights, the results indicate that the policy caused 9.4% more accidents and is statistically significant at the 1% level.[131]

4.5.3.6 Heterogeneous effects

In addition to the average treatment effect that we estimate in our main analysis, we test for measurable heterogeneity in the effect of phone use, for various subgroups of drivers and road characteristics.

We first test whether the effect size varies by age group. Table 4.B.5 in Appendix B suggests that our main effect predominantly applies to drivers in the age group between 30 and 50. We find statistically insignificant effects for age groups below 30 and above 50. However, as the 95% confidence intervals overlap, we cannot conclusively determine that the effects are statistically different, which might be due to less precision. Lab-based studies also tend to be inconclusive on the performance differences of distracted driving across age groups. Oviedo-Trespalacios et al. (2016) synthesise the most recent literature, and find that although "older drivers tend to engage less in a secondary task like using mobile phones while driving [...], the performance of younger drivers, who are inclined to use a mobile phone while driving, has been reported to be less affected by mobile phone tasks than older drivers" (p. 369). It is therefore not surprising that many studies report a negligible effect of age differences.

We also investigate the treatment effect on different road types. Phone distractions may disproportionately impact the likelihood of causing an accident in more challenging road conditions, such as in urban areas and on local roads where drivers often share the road with other vehicles and modes (e.g. pedestrians and cyclists). To test this hypothesis, we split the sample into three road types based on the speed limit. To assure sufficient statistical power, we define the following three road classes with roughly equal numbers of accidents: below 50 km/h, between 50 km/h and 100 km/h, and above 100 km/h. These groups roughly represent local roads in urban areas, local roads in rural areas, and highways. Similarly, we test whether our estimates

[130] This means we have 4,248 province-month observations as compared to 3,688 in column (5) of Table 4.2.

[131] Column (4) of this specification indicates that it indeed appears that the roamer specific time trend is a bad control, as could be expected.

Table 4.3: Estimation results using subsamples of road types and severity.

	log(# Vehicles in Accidents)				
	< 50km/h	50km/h - 100km/h	>100km/h	Fatal/Injury	Material
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.098**	0.050	-0.065	0.115**	0.083**
	(0.038)	(0.040)	(0.049)	(0.054)	(0.033)
log(Hotel nights roamers)	0.178***	0.260***	0.220***	0.130**	0.294***
	(0.058)	(0.078)	(0.045)	(0.051)	(0.075)
log(Hotel nights locals)	-0.031	0.004	-0.125	0.214***	-0.144**
	(0.054)	(0.065)	(0.116)	(0.077)	(0.067)
Weather controls	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes
Weather controls	Yes	Yes	Yes	Yes	Yes
Clusters	72	72	72	70	72
Local vehicles	368k	135k	101k	133k	554k
Roaming vehicles	14k	8k	9k	3k	32k
Observations	3,083	2,796	2,818	2,136	3,636
R^2	0.964	0.955	0.934	0.961	0.965

Notes: Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.

are different for vehicles involved in more severe accidents (fatal or injury) versus accidents with only material damage. Results of these estimations are presented in Table 4.3.

Columns (1–3) indicate that most of the estimated effect comes from local roads, and we do not find evidence of a reduction in road safety on highways. This suggests that phone distractions are either more risky on local roads (e.g. due to crossings and traffic lights), or that drivers use their phone less frequently on highways (e.g. because it is perceived as more dangerous).[132]

Finally, columns (4–5) indicate that the main result holds, regardless of accident severity, suggesting that mobile phone distractions play an important role in accidents with varying degrees of severity. Our results do not support the hypotheses that phone distractions predominantly increase accidents with material damage, for instance, if people mostly use phones in low-speed, low-risk, situations like traffic jams.

[132] We cannot fully isolate the effect of phone usage from that of increased car navigation, but the fact that we find only an effect on urban roads may indicate that car navigation does not increase safety in urbanised areas.

4.5.4 Implications

Our robustness checks indicate that the effect of phone use generally falls within the 95% confidence interval $3\% - 17\%$ of our main estimate. Furthermore, 9.91% is likely to be a conservative estimate of the total effect of phone use because we only estimate the effect induced by the price drop, while roaming users were likely to use their phones, albeit infrequently, prior to the policy. In this section, we calculate the total number of accidents and the relative risk of phone use implied by our main estimate.

4.5.4.1 Total number of accidents caused by phone use

To calculate the number of accidents associated with phone use, we compare the observed number with a counterfactual situation where all drivers face phone usage fees equal to the pre-policy roaming charges. In other words, we consider how many accidents could be avoided if all drivers faced higher phone usage costs and thereby used their phones less. This is a policy-relevant variable because governments can directly affect these costs by, for example, imposing more stringent regulation which increases the costs of being caught using a mobile phone while driving. Importantly, the RLAH policy abolished additional roaming surcharges, such that after the policy, roaming and local users face the same phone use costs and accident risk.[133]

We effectively estimate a local average treatment effect (LATE) of smartphone use while driving, using the RLAH policy as a shifter. The effect is local because some roaming drivers may not comply with the new policy in the sense that they may not increase smartphone usage. If we assume that non-compliance is the same in the treatment and control group, and that the treatment and control group are sufficiently comparable, then we can generalise our LATE to an average treatment effect (ATE) for all road users.

Based on observable driver characteristics, roaming users tend to be younger and drive on faster roads than local drivers (see discussion in Section 4.2.2). Nevertheless, our analysis on heterogeneous effects across age groups suggests that differences in driver age lead to similar results, while highways tend to be safer than local roads with respect to the accident risk of phone distractions (see Table 4.A.3 in Appendix

[133]In other words, the RLAH policy caused roaming drivers to 'catch up' with local drivers' smartphone usage and the distractions and associated accident risk. There may still be variations across mobile phone plans and across countries, but these no longer depend on roaming or local use. In addition, these differences are most likely fairly constant over time in the short run and are more related to local demand and supply conditions than to the RLAH policy.

A). Therefore, based on observable characteristics, our estimates may underestimate the ATE because roaming users are more likely to drive on highways.

One remaining concern might be that unobservable driver characteristics, such as familiarity with roads and other infrastructure, make roaming and local drivers not comparable. For instance, if driving on unfamiliar roads increases accidents risk, then this may be further exacerbated by phone distraction. However, Intini et al. (2018) find no clear evidence for increased accident risk due to unfamiliarity with the road network. On the contrary, they find that familiarity is associated with increased accident risk. More research is required to understand the interaction between driver distractions and road familiarity, but at this stage there seems to be no clear indication that our results overestimate the ATE due to road familiarity.

In sum, it seems plausible that our estimate is roughly similar to the ATE. Our results then imply that phone use causes 13,563 additional accidents annually in the Netherlands, of which about 2,536 result in injury and 79 are fatal. Furthermore, if the ATE is applicable to other EU countries, this would imply that around 2,500 road fatalities in the EU in 2018 may be attributable to phone use.[134] As shown in Figure 4.1, the gap between the EU 2020 target and actual fatalities was 28% (7,044 cases). Our results then suggest that around one-third of this gap could be closed by successfully banning mobile phone use while driving.

4.5.4.2 External effect

We do a back-of-the-envelope calculation to determine the share of drivers that got involved in accidents without being distracted themselves. This can be loosely interpreted as a smartphone-induced increased in the external effect of car use. Let us assume that in each accident, just *one* driver was potentially causing the accident due to phone distraction. Then, out of 764k drivers involved in accidents in our data, 334.89k (43.8%) of them were involved in a crash without contributing to the cause of the accident themselves. If we focus on local roads—where we find the strongest effect of distraction—we find a similar figure of 43.9%.

We use these figures to calculate a simple smartphone-induced increase in the external safety effect of car use, expressed in terms of vehicles involved in accidents. Starting with our main estimate of a 9.91% increase in vehicles involved in accidents due to phone distractions, we calculate that in all accidents, on average about 4.1%

[134] This can be calculated by multiplying our main estimate by the total number of fatal vehicle accidents in 2018, so 9.91% $\times$ 25,058 = 2,470.

of vehicles were affected due to distraction of *other* drivers. Note that this calculation crucially hinges on the assumption that in each phone-induced accident, only *one* driver was distracted. This may seem plausible but may be violated in rare cases.

4.5.4.3 Crash risk odds ratio

We follow Bhargava and Pathania (2013) and translate our estimate for the effect of the change in mobile phone use, due to the RLAH policy, on the number of vehicles involved in accidents to the crash risk odds ratio (or 'relative risk') which allows us to compare our results to the existing literature. This requires two key parameters, the percentage of roaming users that are on their phone while driving or the 'baseline prevalence', and the change in phone use due to the policy, denoted by b and c respectively.

Observational studies, based on roadside surveys, indicate that average phone use in the car ranges between $1 - 11\%$ (European Road Safety Observatory, 2015).[135] These field studies do not distinguish between roaming and local drivers. However, there is a good reason to expect that the baseline prevalence is overestimated for roaming users because roaming was very costly before RLAH. Therefore we consider a range of b $\in$ $[0.01, 0.10]$, in the sensitivity analysis, but note that lower values are more likely.

As for the increase in phone use due to the policy, Table 4.A.4 suggests that RLAH induced an increase in the annual growth rate of mobile data of around 200 percentage points, and calls and texts of around 80 and 20 percentage points, respectively. We assume that aggregate changes in roaming use also apply to drivers visiting the Netherlands and consider a range of c $\in$ $[0.5, 2]$. It is possible that most of this 200 percentage points increase comes from watching videos and playing songs, which may not (fully) translate to an equivalent increase in distractions while driving. This would imply that the lower values in the specified range for c are more relevant and more applicable to our setting.

Using these parameters, we can calculate a range of possible relative risk factors,

[135] Based on a naturalistic driving setting between 2012 and 2015, Dingus et al. (2016) observe handheld cell phone prevalence in the US to be about 6.3%. There is no reason to expect that prevalence is substantially different in the Netherlands, and therefore, we expect that the findings in European Road Safety Observatory (2015) capture a meaningful range for our study.

denoted by RR, implied by our preferred estimate, $\hat{\beta}$, using the formulation:

$$\hat{\beta}[1 \times (1 - b) + RR \times b] = RR \times bc - bc. \tag{4.3}$$

To reflect the uncertainty of these assumptions, Table 4.4 illustrates how our key parameters influence the implied RR estimates. It indicates that RR is decreasing in the baseline prevalence and in the change in phone use due to the policy. In other words, if the policy had a small impact on phone use and roaming drivers used their phone very little prior to the policy, our estimate implies larger risks associated with phone use while driving.

That said, we take a conservative estimate for the baseline prevalence of 3% and the change in phone use due to the policy of 100%. This would imply a relative risk of phone use of 3.8.[136] We consider this to be a conservative estimate as it is unlikely that roaming drivers used their phones as intensively as local drivers due to the high pre-policy roaming costs.

Table 4.4: Sensitivity of implied accident risk.

	Baseline prevalence, b				
Δ phone use due to RLAH, c	1%	2%	3%	5%	10%
50%	17.10	8.90	6.20	4.00	2.30
80%	11.70	6.30	4.40	3.00	1.90
100%	9.80	5.30	3.80	2.60	1.70
150%	7.00	4.00	2.90	2.10	1.50
200%	5.60	3.30	2.50	1.80	1.40

Notes: This table presents the relative accident risk implied by our baseline estimate from column (5) in Table 4.2. The relative risk is calculated by re-arranging equation (4.3) such that: $RR = \frac{\hat{\beta} - \hat{\beta}b + bc}{b(\hat{\beta} + c)}$. Baseline prevalence reflects the percentage of time roaming drivers spend on the phone while in the car. An illustration is outlined in the text.

Comparing these estimates to the existing literature suggests that our conservative estimate of the crash risk associated with modern smartphone usage is similar to earlier crash-based studies, but are significantly larger than recent field studies.[137] This suggests that the crash risks of phone use are slightly lower in magnitude than those found for positive levels of blood alcohol.[138] As mentioned earlier, previous

[136] Re-arranging terms, we can find $RR = \frac{\hat{\beta} - \hat{\beta}b + bc}{b(\hat{\beta} + c)}$. Plugging in $b = 0.03$ and $c = 1$ gives: $RR = 3.8$.

[137] Redelmeier and Tibshirani (1997) find a RR of about 4.3, Dingus et al. (2016) find the RR of cell phone use to be 3.6, and Bhargava and Pathania (2013) do not find any effect. Hersh et al. (2019) do not calculate the RR, however their main estimate of 1.1% is far lower than our main estimate of 9.91%.

[138] Levitt and Porter (2001a) finds a crash risk of 7 and 13 for positive levels of blood alcohol and illegal

research focuses mainly on the effects of calling, or focuses on specific road types and phone use, however, modern smartphones offer substantially more usability and potential for distraction, and our findings suggest that these effects are more likely to be present on local urban roads. Our estimates for the change in mobile phone use due to the RLAH policy suggest that we mainly pick up an effect from using more mobile data (increase in the growth rate of about 200 percentage points as compared to local drivers) which may explain why we find larger implied relative risk estimates than some earlier field studies.

4.6 Conclusion

In this study, we provide novel evidence on the effect of cell phone use on car accidents. We exploit variation in the cell phone usage fees in the Netherlands following the *Roam Like at Home* (RLAH) policy introduced by the European Union (EU) in 2017. This intervention is used as a treatment, and applies to roaming users—non-Dutch drivers from the EU—, which allows us to employ a difference-in-differences approach.

We show that the growth rate of mobile calls, texts, and particularly data usage increased substantially after the change in roaming regulations, making roaming phone use more in line with usage in home countries. While we do not directly observe actual phone use of drivers, the observed increase in usage is likely to (partly) carry over to phone use while driving. We estimate that decreased smartphone usage fees lead to an increase in the number of vehicles involved in accidents of 9.91% (95% confidence interval $3\% - 17\%$). This is likely to be an underestimate of the *total effect* of phone use while driving, as our estimates capture the effect of an *increase* in smartphone use, which was not fully absent before the policy.

Under the assumption that the identified mechanism carries over to all EU drivers, our estimate implies that, in 2018, around 2,500 road fatalities in the EU could be attributed to phone use. Our results then suggest that around one-third of the gap between realised safety improvements on roads and the EU 2020 target can be attributed to mobile phone use.[139]

Our findings indicate that the existing literature may underestimate the risks associated with modern smartphone usage while driving. Our main result implies a crash risk odds ratio associated with mobile phone use of around 3.8, which is likely to be

levels respectively.

[139] In 2018, the EU was 28% away from their 2020 target (see Figure 4.1).

a conservative estimate. All in all, our results suggest that smartphones are making roads less safe, and this has important implications for road safety policies.

Our paper provides an estimate of the *average* effect of smartphone usage on the number of vehicles involved in traffic accidents, which may conceal considerable differences between specific groups of drivers. We look into heterogeneous effects by estimating models for different sub-samples (e.g. for different age groups, or excluding trucks). Future research could delve into this further, by estimating propensities of specific groups of drivers to use their phone while driving. Ride-hailing drivers, for example, may have a relatively high propensity to be distracted by their phone, which might be an important factor in explaining the results of Barrios et al. (2020), who find that ride-hailing services increased the number of traffic accidents in the US. Such evidence could provide valuable input for related regulation and policies.

Appendix 4.A Additional descriptives

Table 4.A.1: Descriptive statistics: Vehicles in accidents.

Statistic	N	Mean	St. Dev.	Min	Max
Roaming	764,065	0.046	0.210	0	1
Age	561,136	42.488	15.015	0.000	110.000
Female	764,065	0.455	0.707	0	10
Maximum speed (km)	653,055	63.726	26.823	15.000	130.000
Deadly	764,065	0.006	0.076	0	1
Injury	764,065	0.187	0.390	0	1
Material	764,065	0.807	0.394	0	1

Table 4.A.2: Descriptive statistics by group: Vehicles in accidents.

Variable	Roaming	Local	Diff	Tstat
Age	40.903	42.566	-1.663	18.998
Female	0.383	0.459	-0.075	21.007
Maximum speed (km)	74.511	63.200	11.312	-62.898
Deadly	0.006	0.006	-0.000	0.503
Injury	0.088	0.192	-0.103	65.301
Material	0.906	0.802	0.104	-63.736

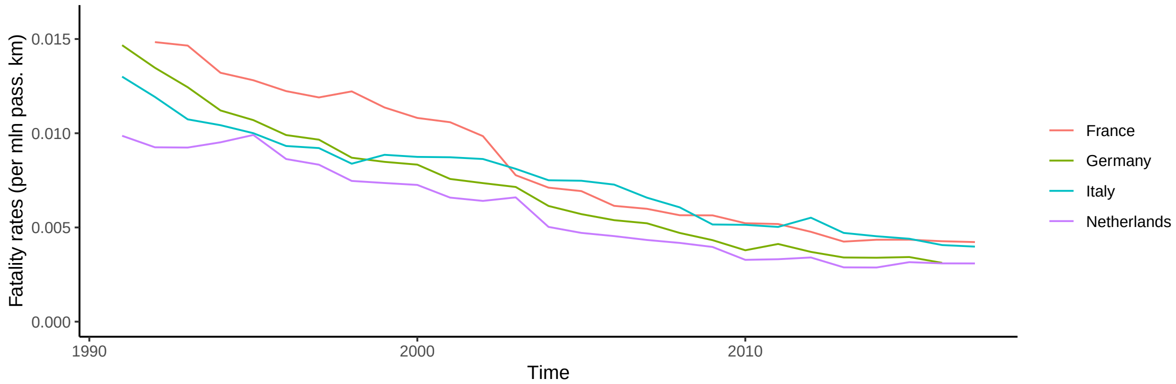


Figure 4.A.1: Fatality rates in road accidents over time in major EU countries and the Netherlands

Table 4.A.3: Relative frequencies of road types by severity

	Local roads	Major roads	Highways
Fatal/injury	14.1 %	4.3 %	2.0 %
Material damage	46.8 %	17.9 %	14.8 %
Total	60.9 %	22.2 %	16.9 %

Table 4.A.4: Difference-in-differences in annual growth of phone use.

		Annual growth rate (%)		Δ Annual growth rate (p.p.)	
Usage	User	Pre	Post	Diff	DiD
Calls	Local	4.29	-2.49	-6.78	
Calls	Roaming	-1.06	71.16	72.21	78.99
Data	Local	67.05	83.82	16.76	
Data	Roaming	68.09	285.89	217.80	201.04
Texts	Local	-18.24	-1.80	16.45	
Texts	Roaming	-22.01	18.37	40.38	23.93

Notes: Pre-policy refers to the the average annual growth rates of cellular traffic comparing each quarter with the same quarter in the previous year, over three years (Q1 2014 − Q1 2017) prior to the implementation of RLAH. Post-policy is one year, Q2 2017 − Q1 2018, after RLAH.

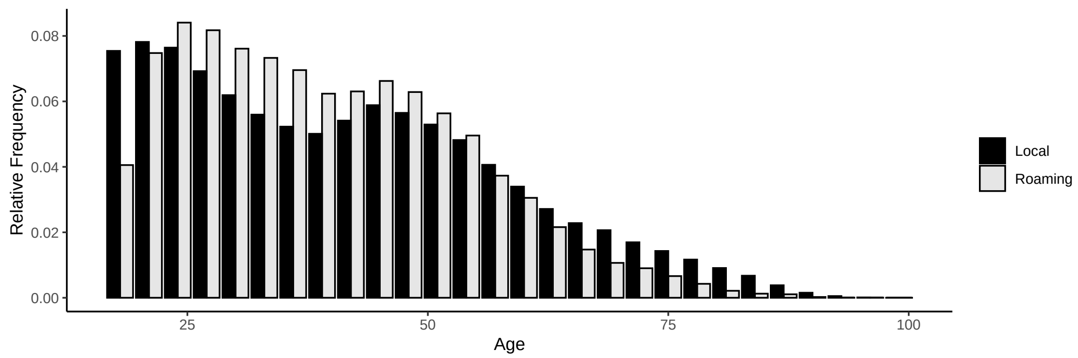


Figure 4.A.2: Age of local and roaming users.

4.A.1 Predicting phone use per subscriber pre-2016

We obtain roaming usage data from the EU Body of European Regulators for Electronic Communications (BEREC). Their reports include the time series "EEA average consumption per month per total number of roaming subscribers (in GB)" from the second quarter of 2016 onwards. Therefore, in order to get a better picture of the long

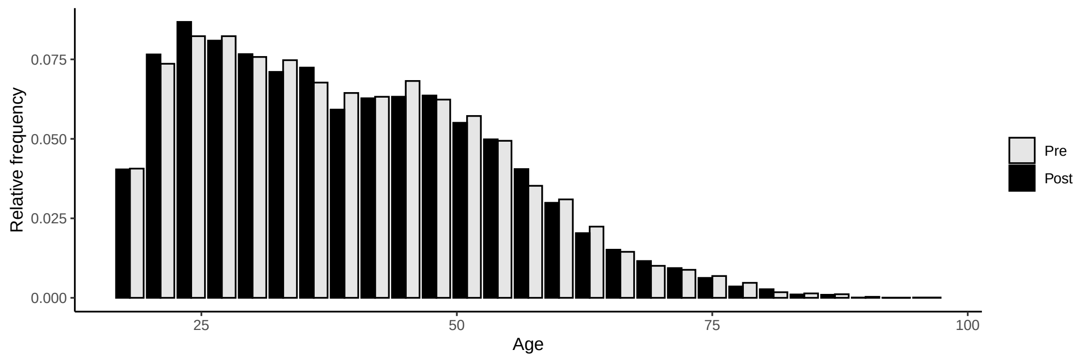


Figure 4.A.3: Age of roaming users pre and post policy.

term changes in roaming data usage, we use data on the total "EEA Retail data traffic (millions of GB)" (available as of 2007) and predict the number of subscribers in earlier periods using a simple model. The advantage of this approach is that the number of subscribers appears to follow a rather simple dynamic process and means that we only need to predict the denominator. We can then also compare the growth in our metric to the total growth in mobile data use which gives us more confidence that the predictions are as close as possible to actual figures.

We observe quarterly data on the number of roaming subscribers from the second quarter of 2016 until the first quarter of 2019. The top panel in Figure 4.A.4 indicates that the number of subscribers appears to follow a somewhat log-linear growth trend with a strong seasonal pattern which is likely related to summer tourism. We therefore estimate the number of subscribers using the following regression equation:

$$\log(S_t) = \gamma \text{Trend}_t + \phi_{q(t)} + \epsilon_t, \tag{4.A.4}$$

where $\log(S_t)$ is the natural logarithm of the number of subscribers, Trend_t is a linear time trend capturing the growth over time, and $\phi_{q(t)}$ are quarter dummies that capture seasonal variations. The resulting model has an $R^2 = 0.92$, which suggests that it captures the vast share of roaming subscriber dynamics. This is further confirmed by the bottom panel of Figure 4.A.4 which compares the actual and predicted number of subscribers and the resulting calculation of data roaming per subscriber. Finally, Figure 4.A.5 compares the difference between growth in roaming data per subscriber and the total roaming data use. While the trends are almost identical, it indicates a larger growth in total data use which is likely a result of capturing overall trends in growth in subscribers (which is relatively constant) and may also be a result of the RLAH policy that causes the number of people actively using roaming while travelling to increase. Overall, it suggests that the predicted change in data usage is a conservative estimate of the effect of the policy.

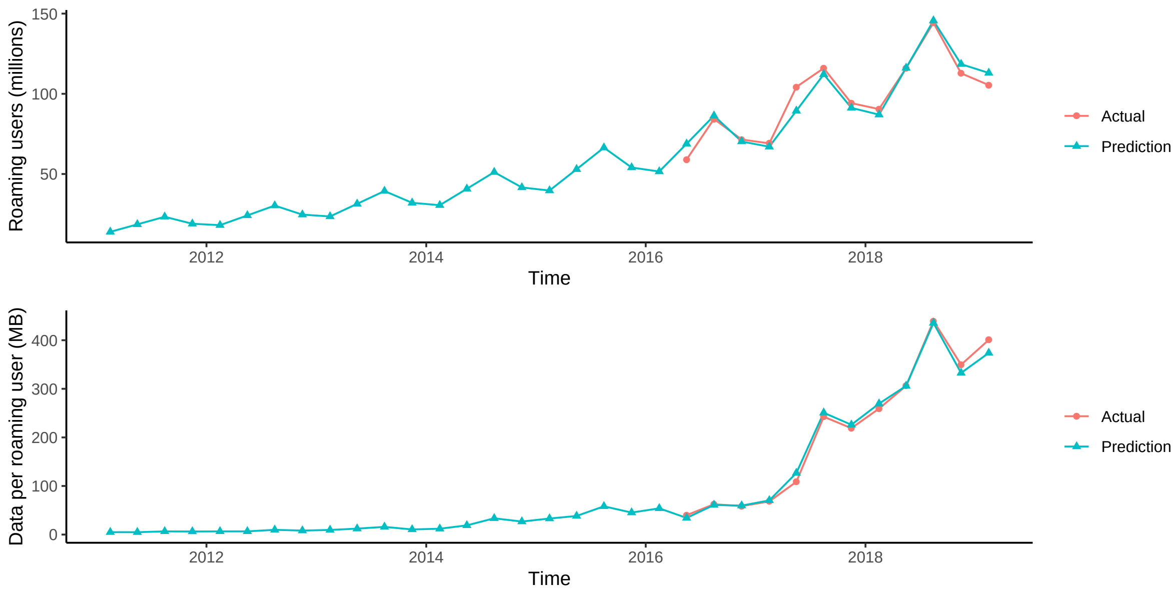


Figure 4.A.4: Predicting number of EU roaming subscribers and data consumption

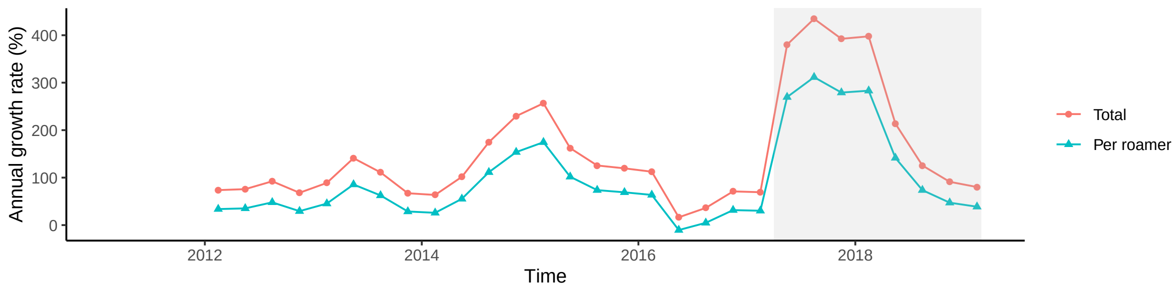


Figure 4.A.5: Growth in roaming data usage per subscriber as compared to the total

4.A.2 Analysis of hotel nights as proxy for vehicle kilometres travelled

Table 4.A.5: Regression results for analysing traffic and hotel nights for Dutch drivers.

	log(Traffic intensity)		log(# Vehicles in accidents)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(Hot. loc.)	0.241**	0.043	0.787***	0.114			0.629***	-0.132*
	(0.108)	(0.065)	(0.117)	(0.084)			(0.070)	(0.075)
log(Hot. roam.)					0.288***	0.322***	0.303***	0.311***
					(0.086)	(0.077)	(0.082)	(0.078)
Time FE		Yes		Yes		Yes		Yes
Panel FE		Yes		Yes		Yes		Yes
Subsample	Loc.	Loc.	Loc.	Loc.	Roam.	Roam.	All	All
Within R2	0.233	0.017	0.660	0.025	0.241	0.046	0.806	0.047
Observations	564	564	564	564	2,980	3,688	3,688	3,688
R^2	0.233	0.992	0.660	0.990	0.241	0.966	0.806	0.966

Notes: Robust standard errors in parentheses are clustered at the province and country-group level. Hotel nights are split into two orthogonal variables for local and roaming users. An additional dummy is included when hotel nights were inflated (only occurs for roaming users).***, **, * indicate significance at 1%, 5%, and 10%.

Appendix 4.B Additional results

Table 4.B.1: Results correcting for sources of measurement error

	log(# Vehicles in Accidents)				
	No trucks	SV	No border prov.	No border countr.	No BG/PL/RO
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.086**	0.096***	0.072*	0.134***	0.077**
	(0.034)	(0.033)	(0.037)	(0.030)	(0.034)
log(Hotel roam.)	0.297***	0.147**	0.424***	0.132***	0.335***
	(0.093)	(0.056)	(0.121)	(0.037)	(0.084)
log(Hotel loc.)	-0.117*	-0.167***	0.087	-0.038	-0.071
	(0.061)	(0.054)	(0.082)	(0.072)	(0.064)
Weather controls	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes
Clusters	72	72	30	48	60
Local vehicles	535k	136k	356k	686k	686k
Roaming vehicles	22k	6k	12k	15k	26k
Observations	3,303	2,658	1,523	2,458	3,026
R^2	0.966	0.962	0.968	0.977	0.972

*Notes:*Robust standard errors in parentheses are clustered at the province and country-group level. BG/PL/RO refer to Bulgaria, Poland and Romania respectively, which can be considered to be labour migration countries.***, **, * indicate significance at 1%, 5%, and 10%.

Table 4.B.2: Results using only one year pre/post (1–2), and data aggregated to two periods (3–4).

	log(# Vehicles in accidents)			
	All	Single vehicle	All	Single vehicle
	(1)	(2)	(3)	(4)
Treatment effect	0.065**	0.131***	0.148**	0.162*
	(0.029)	(0.036)	(0.070)	(0.091)
log(Hotel nights roamers)	0.316***	0.192***	0.114	-0.082
	(0.094)	(0.064)	(0.094)	(0.109)
log(Hotel nights locals)	0.036	-0.053	0.148	-0.037
	(0.069)	(0.078)	(0.424)	(0.409)
Weather controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes
Clusters	72	72	72	72
Local vehicles	319k	59k	23k	4k
Roaming vehicles	18k	3k	1k	0k
Observations	1,593	1,162	144	143
R^2	0.969	0.962	0.999	0.998

Notes: Robust standard errors in parentheses are clustered at the province and country-group level. Columns (1–2) are obtained using data from June 2016 until July 2018. Columns (3–4) are obtained after aggregating the data into two periods, one before the policy and one after. For interpretation purposes, after aggregation, variables are then rescaled to their initial units (e.g. monthly averages).***, **, * indicate significance at 1%, 5%, and 10%.

Table 4.B.3: Regression results using weighted least squares.

	log(# Vehicles in Accidents)			
	(1)	(2)	(3)	(4)
Treatment effect	0.111***	0.128***	0.110***	0.101***
	(0.030)	(0.033)	(0.028)	(0.037)
log(Hotel nights roamers)	0.196***	0.185***	0.255***	0.256***
	(0.061)	(0.057)	(0.059)	(0.092)
log(Hotel nights locals)	-0.212***	-0.173*	-0.083	-0.221*
	(0.078)	(0.094)	(0.066)	(0.118)
Weather controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes
Weights	Total veh.	Roaming veh.	Avg traf. intens.	Avg hotel nights
Clusters	72	72	72	72
Local vehicles	686k	686k	686k	686k
Roaming vehicles	35k	35k	35k	35k
Observations	3,688	3,688	3,688	3,688
R^2	0.970	0.969	0.966	0.968

Notes: Estimated using weighted least squares, with pre-policy total number of (roaming) vehicles as weights. Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.

Table 4.B.4: Estimation results using Poisson regression.

	# Vehicles in Accidents				
	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.186***	0.186***	0.187***	0.0381	0.0899***
	(0.0234)	(0.0210)	(0.0223)	(0.0262)	(0.0298)
Roamer × trend				0.00520***	
				(0.000921)	
log(Hotel nights roamers)					0.335***
					(0.0709)
log(Hotel nights locals)					-0.000314
					(0.0689)
Temperature		0.0366	-0.00100	-0.00101	-0.00108
		(0.0264)	(0.000852)	(0.000851)	(0.000850)
Rain		0.198**	0.0134***	0.0132***	0.0139***
		(0.0788)	(0.00447)	(0.00443)	(0.00452)
# Frost days		0.0571	-0.000380	-0.000526	-0.000896
		(0.0631)	(0.00301)	(0.00297)	(0.00352)
Time FE	No	Yes	Yes	Yes	Yes
Panel FE	No	No	Yes	Yes	Yes
Clusters	72	72	72	72	72
Local vehicles	686	686	686	686	686
Roaming vehicles	35	35	35	35	35
Observations	4,248	4,248	4,248	4,248	4,248

Notes: Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.

Table 4.B.5: Estimation results for subsamples with different age groups.

	log(# Vehicles in Accidents)					
	All	Age ≤ 30	$30 <$Age< 50	Age≥ 50	Age≥ 65	Age unknown
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	0.094***	0.039	0.099**	0.058	0.055	0.193**
	(0.031)	(0.038)	(0.039)	(0.035)	(0.051)	(0.087)
log(Hotel nights roamers)	0.298***	0.201***	0.200***	0.204***	0.027	0.181**
	(0.076)	(0.039)	(0.050)	(0.064)	(0.049)	(0.072)
log(Hotel nights locals)	-0.089	0.058	-0.046	0.016	0.063	-0.238
	(0.063)	(0.070)	(0.080)	(0.069)	(0.096)	(0.166)
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel FE	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	72	72	72	72	62	72
Local vehicles	686k	189k	221k	172k	59k	104k
Roaming vehicles	35k	7k	12k	6k	1k	9k
Observations	3,688	2,638	3,072	2,572	1,422	2,822
R^2	0.967	0.959	0.954	0.959	0.970	0.924

Notes: Robust standard errors in parentheses are clustered at the province and country-group level.***, **, * indicate significance at 1%, 5%, and 10%.

5

Automobiles and urban density

5.1 Introduction

A defining feature of urbanisation in the 20$^{\text{th}}$ century has been the introduction and rapid, wide-scale adoption of the the automobile. By lowering marginal transport costs and eliminating the need to walk almost entirely, cars allow people to travel longer distances with greater flexibility in terms of routes and schedules. As Henry Ford predicted, this facilitated the decentralisation of cities via the outward expansion of people and firms into the periphery, where land is cheaper, thereby radically changing urban form (Anas et al., 1998; Baum-Snow, 2007; Baum-Snow et al., 2017).

Urban population density is perhaps the most distinguishing characteristic of a city and is a common measure of urban form. Higher density is associated with positive agglomeration economies, public transport efficiency and urban amenities (see Ciccone and Hall, 1996; Glaeser et al., 2001; Rosenthal and Strange, 2004; Glaeser et al., 2008), while lower density linked with higher pollution levels, environmental damage, obesity and segregation of rich and poor (see Brownstone and Golob, 2009; Eid

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et al., 2008; Zhao and Kaestner, 2010; Couture et al., 2019; Gaigné et al., 2020). Therefore, studying the effect of automobiles on urban density is important in the light that, in most countries, cars are subsidised, implying that car ownership is too high and therefore urban densities may be too low from a welfare perspective (Parry et al., 2007; Au and Henderson, 2006; Brueckner and Helsley, 2011).[140]

In spite of the relevance of this topic, Glaeser and Kahn (2004) argue that we know very little about the long-run effect of car ownership on urban density. This knowledge gap is likely related to the econometric challenge for causal inference and the lack of a consistent dataset on urban and transport indicators. The first challenge is reverse causality: residents are more likely to use a car in cities with lower urban densities, therefore car ownership rates in these cities may be higher (Bento et al., 2005b; Duranton and Turner, 2018; Ewing et al., 2018). Hence, one may overestimate the causal (negative) effect of cars on density if reverse causality is ignored. The second challenge is that urban density is highly persistent over time and is correlated to many difficult-to-observe factors (for example land use planning). So, in order to identify the causal long-run effect of cars on urban density, one requires a *long-term* exogenous shock in car ownership.

We address these challenges using an IV strategy and by leveraging the best available global dataset of large cities constructed primarily by Kenworthy and co-authors over various waves (Ingram and Liu, 1999; Kenworthy and Laube, 2001; UITP, 2015). As an instrument we use the presence of a domestic commercial car manufacturer in 1920, hence when few people owned cars. We provide evidence that countries with a historic car manufacturer currently still pay lower prices for car use and ownership through lower taxation and more roads. Furthermore, we will show that the presence of manufacturer in 1920 is uncorrelated to urban density around that time, which supports our argument that historic car manufacturers are a plausible instrument for car ownership.[141]

Our research design is inspired by Glaeser and Kahn (2004) who were, to our knowledge, the first (and only) to study the causal effect of car ownership on urban density. Using legal origin as an instrument for car ownership, they conclude that cities with lower car ownership rates tend to have higher urban densities. Their main estimate indicates that one additional car per 100 inhabitants is associated with a reduction

[140] For example, in Europe, about 40 percent of all new cars are subsidised through distortionary company car taxation (Van Ommeren and Wentink, 2012), whereas congestion, safety, and environmental externalities are only partially included in the overall price of car use in the US (Parry and Small, 2005).

[141] Up to the extent that one is still concerned that omitted variables bias is an issue, we also estimate fixed-effects models and use the methodology proposed in Oster (2019) to show that our baseline OLS and IV estimates are conservative.

in urban density of 7.2%. However, as the authors acknowledge themselves, given their limited dataset and identification strategy, these results should be interpreted as suggestive.[142] Our main contribution is to improve on their analysis, by expanding the dataset considerably, introducing a new identification approach, and performing a more extensive set of robustness checks. Although for some cities we have observations for several periods, we treat the data as a cross-section and apply our main identification strategy for 232 city observations in 123 cities and 57 countries between 1960 and 2012.[143]

Our work is closely related to a large literature studying the effects of transport infrastructure on the spatial distribution of people and jobs (Baum-Snow, 2007, 2010; Garcia-López et al., 2015; Baum-Snow et al., 2017; Levkovich et al., 2017; Heblich et al., 2018; Gonzalez-Navarro and Turner, 2018). This literature demonstrates that highways are an important driver of decentralisation in the 20$^{\text{th}}$ century, while subways only had a moderate impact. However, highways explain only a portion of car-induced decentralisation.[144] Various other policies, such as vehicle taxes, fuel taxes, and parking regimes, effect car ownership and use, and thereby urban density, so estimates of the effect of highways only give a partial view.[145] Hence, we aim to obtain insight into the overall effect of the automobile, captured by a comprehensive measure such as car ownership, which is the focus of this paper.

The results indicate that one additional car per 100 inhabitants reduces population and employment density by around 2.4% in the long-run. This effect appears to be mainly driven by expansions in the built-up area, and not by population leaving the city, suggesting that cars facilitate low density urban development in the periphery. We use these estimates to gauge the potential effects of growing car ownership rates in developing countries and the introduction of automated vehicles. Applying these estimates, for example, to developing Asian cities indicates that if car ownership increases to similar rates as seen in high-income countries, urban density may fall by over 50% in the long-run. Our estimates are also relevant for high-income countries with low car ownership rates (for example Denmark) as automated vehicles will likely increase access to cars and thereby, in the absence of policy, cause cities to decentralise.

The paper proceeds as follows. In Section 5.2 we introduce the data and provide some descriptives. In Section 5.3 we elaborate on the methodology. We report the main results and discuss some implications in Section 5.4 and finally, Section 5.5 concludes.

[142] Glaeser and Kahn (2004) employ data from Ingram and Liu (1999), which contains 70 observations for 35 cities in 18 countries (in 1960 and 1980).

[143] Cities that are observed multiple times are weighted by the inverse of the number of cities.

[144] Evidence will be provided in Appendix 4.

[145] For example, in Norway there are few highways while car ownership is high.

5.2 Data and context

5.2.1 Data

We use several sources of information. The most important source is city level data on population, employment, area size, income and transportation, between 1960 and 2012. We obtain data for 1960 and 1980 from Ingram and Liu (1999), which comprises 69 observations from 35 cities in 18 countries.[146] Data for 1995 comes from the Millennium Cities Database (henceforth MCD1995) and contains information on 100 cities from 51 countries (Kenworthy and Laube, 2001; Kenworthy, 2017).[147] Finally, we also obtain data for 2012 from the Mobility in Cities database (henceforth MCD2012), collected by UITP (2015) using the same methodology as MCD1995, which includes 63 cities from 39 countries. A key advantage of these sources is that they rely on a consistent methodology for data collection, which allows us to make accurate comparisons between cities from different countries and time periods. Most importantly, the metropolitan area is consistently defined as the 'commuter belt or labour market region' for all our data and hence captures the bulk of home-work journeys in a city.[148]

Population density is our main measure of urban structure, but we also examine other measures such as employment density and centrality of employment.[149] Population density is measured as the total population in a metropolitan area divided by the total built-up area (in km^2).[150] It therefore captures the density of developed land, accounting for geographical factors such as water and green space which may limit density.

These data sources additionally include information on car ownership per capita, metropolitan GDP per capita, highway length, and the MCD1995 dataset also contains car-related variables such as the average cost of a car trip, annual capital costs of a car, and the number of kilometres of roads and highways. We also collect climate

[146] 25 cities (mainly in developed countries) are collected from Newman and Kenworthy (1989) and are supplemented with 10 other (mostly developing) cities, from various sources. We remove the observation of Guangzhou in 1994 as it overlaps almost exactly with the MCD1995 data.

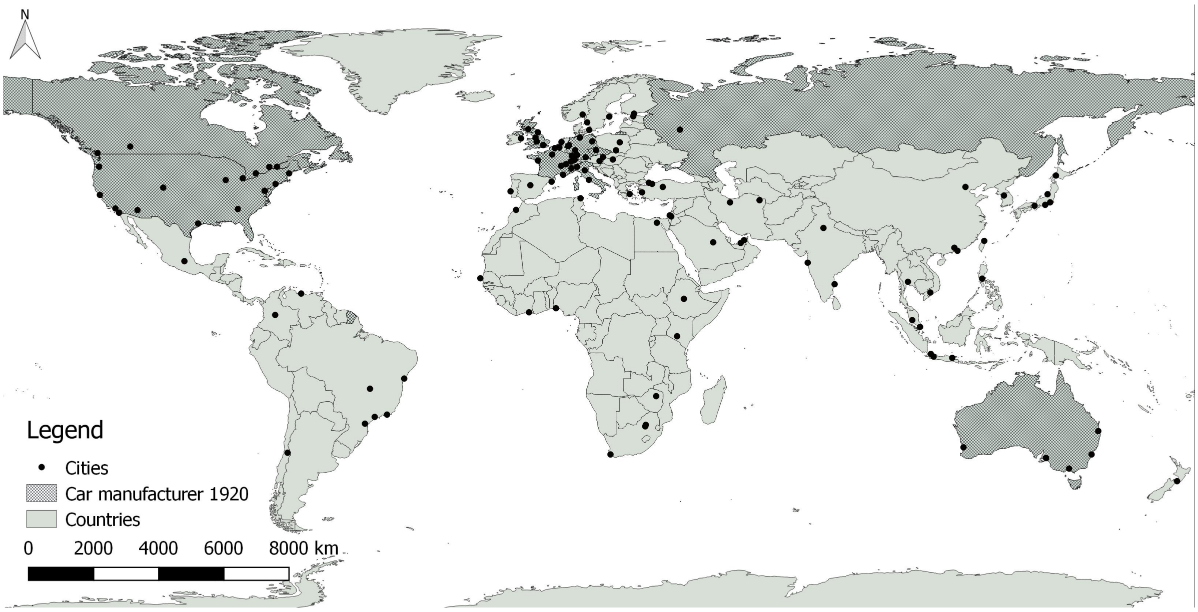
[147] The dataset contains full information for 89 cities in 41 countries. We impute the missing data points for 11 cities to obtain a complete dataset of 100 cities in 51 countries. In four cities built-up area was missing. In an additional four cities metropolitan GDP was missing and in three cities population density in 1920 was missing. Appendix 1 documents how we impute these data points.

[148] This is comparable to the OECD (2013) definition of 'functional urban areas', including the hinterland or 'worker catchment area'.

[149] Employment information is only observable for MCD1995 and MCD2012.

[150] Built-up area includes gardens and local parks, urban wasteland, transport infrastructure, recreational, residential, industrial, office, commercial, public utilities, hospitals, schools, cultural areas and sports grounds.

Figure 5.1: Cities in our dataset



and elevation data from Fick and Hijmans (2017) and Reuter et al. (2007) using longitude and latitude coordinates of each city centroid. This allows us to compute average January and July temperatures, annual precipitation, altitude, and ruggedness of the terrain.[151]

To construct our instrument, we collect information on whether a country had a domestic commercial car manufacturer in 1920 by cross-referencing the *Timeline of Motor Vehicle Brands* (Wikipedia, 2018). We focus on 1920, because car ownership rates were still very low, however car manufacturers started to have political influence for an extended period of time. Therefore, we are mainly interested in domestic commercial car manufacturers that may have had substantial political leverage over an extended period of time. Hence, we exclude subsidiaries, niche or boutique car companies, and countries that had car manufacturers for only a short period after 1920.[152] In Appendix 3, we document which car manufacturers were present in each specific country, the year they opened and (if relevant) the year they closed down, including primary sources. To complement our historical instrument, we also collect historical country-level data on population and GDP per capita for the year 1913, just before

[151] We use a similar measure as Burchfield et al. (2006) and calculate ruggedness as the standard deviation of altitude within 50 km from a city's centroid.

[152] For example, we exclude a subsidiary of the Ford Motor Company founded in 1919 (Brasil), a luxury car manufacturer called "Hispano-Suiza" founded in 1904 (Spain), and the Austro-Hungarian manufacturer "Austro-Daimler" which operated from 1899 until 1934, and therefore was not present over a substantial portion of the twentieth century.

Table 5.1: Descriptive statistics

	N	Mean	Std. dev	Min	Max
Population density (pop/km^2)	232	7342.44	6473.72	530.00	35564.53
Employment density (jobs/km^2)	144	3169.18	2712.12	280.00	15127.67
Cars per capita	232	0.32	0.20	0.00	0.84
Car km per capita (100 km)	123	41.90	37.45	0.36	201.97
GDP per capita ($1000s)	232	22.24	21.18	0.22	104.63
Population (millions)	232	4.23	4.92	0.24	37.24
Total built-up area (km^2)	232	874.98	1217.04	44.29	10657.15
Total surface area (km^2)	153	4293.09	8043.50	126.09	57378.00
Built-up to surface area	153	0.36	0.23	0.03	0.93
January temperature (°C)	232	9.31	10.26	-10.40	27.60
July temperature (°C)	232	21.47	5.68	8.80	35.30
Annual precipitation (m)	232	0.97	0.56	0.03	2.93
Altitude (km)	232	0.39	0.53	-0.00	2.60
Ruggedness	232	0.18	0.22	0.00	1.68
Car manufacturer 1920	232	0.37	0.49	0.00	1.00
Country Pop 1913 (millions)	232	49.63	88.85	0.08	437.14
Country GDP per capita 1913 ($1000s)	232	3.77	2.67	0.38	8.38
Pop dens. 1920 (pop/km^2)	232	8689.51	9900.78	1308.93	77736.80
Source: Ingram & Liu 1960	232	0.06	0.24	0.00	1.00
Source: Ingram & Liu 1980	232	0.11	0.32	0.00	1.00
Source: MCD 1995	232	0.53	0.50	0.00	1.00
Source: MCD 2012	232	0.29	0.46	0.00	1.00

WWI began, from Bolt et al. (2018).[153] Finally, we construct a measure for population density for a representative city in 1920 using data from Goldewijk et al. (2017), as this information is not available using existing sources.[154]

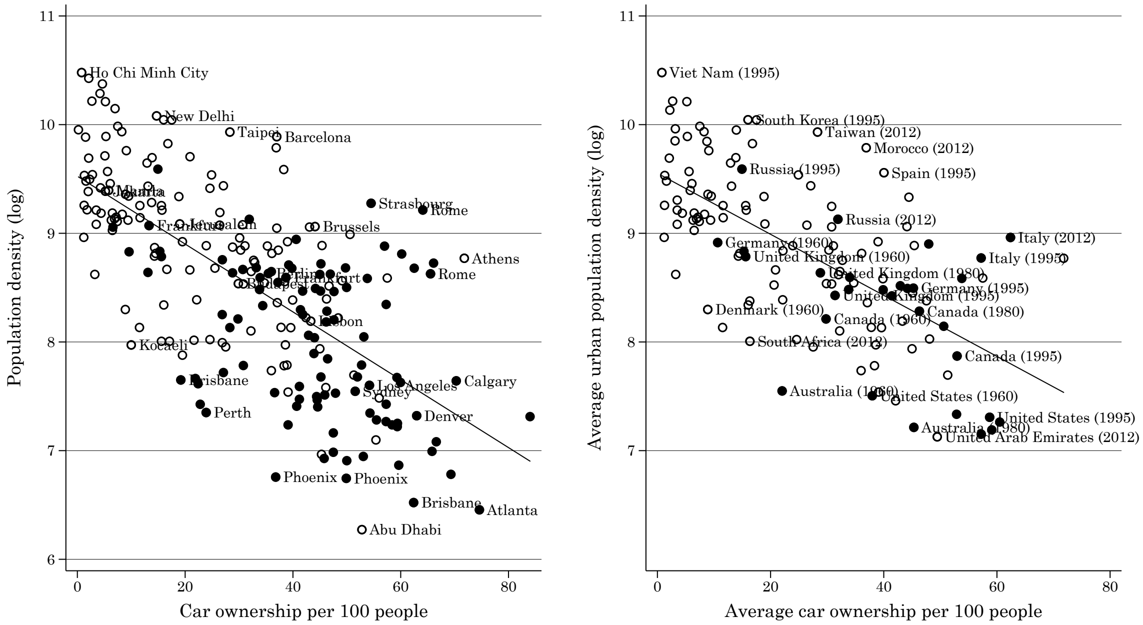
5.2.2 Descriptive statistics

Descriptive statistics are provided in Table 5.1. Population density is around $7,300$ people per km^2 and the number of cars per capita is 0.32, on average. An average city is large, containing a population of around 4.2 million and spanning a built-up area of around 870 km^2, about 36% of the total city surface area. About one third of

[153] Most car manufacturers were present before 1920, therefore it seems appropriate to use information for other historic variables slightly prior to 1920.

[154] We describe the procedure to calculate 1920 population density in Appendix 2. All data are available upon request.

Figure 5.2: Population density and car ownership per capita



Notes: The left plot shows city-year data and the right plot shows country-year data. Y-axis is log scaled. Filled (black) circles represent countries with a car manufacturer in 1920. City and country labels are based on minimum, median, and maximum population densities for each bin of 10 cars per 100 people. The solid line represents the bivariate linear regression.

cities are located in counties with a car manufacturer in 1920. We provide additional histograms of the main variables of interest in Figure 5.A.2 of Appendix 4. They show, for example, that most cities have population densities below $10,000$ people per km^2, with a median of around $5,600$.

The cities we focus on are rather large and represent labour market areas. Hence, employment density is highly correlated to population density (the correlation is 0.92). In the analysis, we focus on the effect of car ownership on population density and we repeat the main specifications with employment density and other measures in Section 5.4.3. Figure 5.2, left plot, shows that the bivariate relation between car ownership per capita and population density of cities is approximately log-linear and strongly negative. The right plot confirms that message when aggregating our data at the country level.[155] It also shows that countries with historic car manufacturers have notably higher rates of car ownership and lower population densities. In line with common knowledge, US and Australian cities tend to have the highest rates of

[155] We calculate the urban density in a country, by just considering population in and area size of urban areas in our sample.

car ownership and lowest urban densities.

5.3 Empirical methods

We aim to estimate the long-run causal effect of car ownership per capita on population density. Indexing city i in country j at time t, we set up the following regression equation:

$$\log(D_{ijt}) = \alpha + \beta C_{ijt} + \gamma X_{ijt} + \zeta G_j + \phi_t + \epsilon_{ijt}, \tag{5.1}$$

where $\log(D_{ijt})$ is the natural logarithm of population density, C_{ijt} represents car ownership per capita, X_{ijt} and G_j are vectors of observed city and country characteristics, ϕ_t are decade fixed effects, and ϵ_{ijt} is an error term. Note that for some cities, we have more than one observation. We address this issue by using weights, with weights inversely proportional to the number of observations per city. As observations do not come from exactly the same year, we control for decade fixed effects, ϕ_t, in all specifications.[156] For all estimates, we cluster standard errors at the country level.

Estimating the marginal effect β with OLS gives consistent estimates of the causal effect of car ownership on urban density, provided that $\text{cov}(C, \epsilon|X, G) = 0$. There are at least two endogeneity concerns when estimating equation (5.1) by OLS. First, we may omit important variables which affect both population density and car ownership. Second, changes in the urban structure may lead to changes in mobility, resulting in reverse causation as cities with lower densities may induce more car ownership which may in turn cause lower densities (Duranton and Turner, 2018).

To tackle the first issue we include a range of important controls. Higher incomes are correlated with higher rates of car ownership (Dargay, 2002) and lower population densities as people demand more space to live in, therefore, we control for the log of GDP per capita at the city level (Margo, 1992).[157] Geographical factors such as the climate, altitude and ruggedness of terrain might also effect car ownership and urban density, for example because larger gardens are more attractive in warmer climates and construction is more expensive in hilly terrain, while active modes of transport

[156] This also controls for data source, as each source comes from a different decade.

[157] Note, whether richer households chose to live closer or further from the city centre depends on the sign of the housing and commuting elasticity with respect to income. In European cities, where the urban core is characterized by strong residential and workplace amenities, richer households are likely to locate closer to the centre (Brueckner et al., 1999; Gutiérrez-i Puigarnau et al., 2016).

like cycling are less likely (Burchfield et al., 2006). Therefore we control for January and July temperatures, precipitation, altitude and ruggedness. The regulatory environment and cultural factors may also play a role in determining attitudes towards car ownership and urban planning (Duranton and Puga, 2015). La Porta et al. (1999, 2008) argue that legal origins influence a broad range of rules and regulations and find that civil law countries tend to be more regulated than common law countries. We therefore control for English, French, German, and Scandinavian legal origins to capture potential correlation between land-use and vehicle regulations which may affect both population density and car ownership.

To examine omitted variable bias of OLS estimates, we also perform a bias-correction approach which allows us to place a bound on the OLS estimate of β, denoted β_{OLS}, in the presence of omitted variables. Oster (2019) shows that a consistent estimate of the bias-adjusted treatment effect can be calculated given assumptions on two key parameters: (*i*) the proportion of car ownership explained by unobservables relative to observables, δ, and (*ii*) the maximum variation in the log of population density that can be explained by observables and unobservables, $R^2_{\max}$.[158] To further address concerns related to omitted variable bias, we also estimate fixed-effects models in Section 5.4.5.1.

In order to tackle the issue of reverse causality, we require a long-term exogenous shock in the use of automobiles. Glaeser and Kahn (2004) apply an IV approach, using legal origin (French civil law) as an instrument for car ownership, so identification is based on country differences.[159] Legal origin may be argued to be a plausible instrument as it pre-dates the invention of the car and countries with French civil law tend to be more regulated, hence face higher vehicle costs. However, as mentioned above, one criticism is that because countries with French legal origins tended to have more regulation, the instrument may also impact urban density directly via other stricter regulations such as urban planning (La Porta et al., 2008).[160] Another issue is that in our dataset, the instrument appears to be weak (the *F*-statistic is 2.78).

We therefore propose the presence of a domestic commercial car manufacturer in a country in 1920 as an alternative instrument for car ownership per capita.[161] In the

[158] Oster (2019) recommends to use $R^2_{\max} = 1$ as an upper bound, which implies that any bias will be overstated. Our estimator sometimes delivers multiple solutions if the importance of unobservables is low, *i.e.* $\delta \leq 1$. We then select the solution closest to β_{OLS}, as the alternative solution provides outlier estimates that are not in line with any other specification. In case that the importance is high, *i.e.* $\delta > 1$, which is the more interesting case, we only obtain single solutions.

[159] We collect data on legal origin from Appendix B in La Porta et al. (1999).

[160] For example, Titman and Twite (2013) find that a countries' legal origin is correlated to the building's lease duration and to the number of high-rise office buildings, and therefore affects urban density directly.

[161] The presence of a car manufacturer at the city level is less likely to be exogenous for two reasons.

1920s, few people owned cars. At that time, the US led the world in car manufacturing and ownership. Nevertheless, in the US there were only 8 million registered cars and the ownership rate was only 0.08.[162] Except for in the US, car ownership and the construction of highways only started to rise in the world after the 1950s. As car ownership began to rise, so did the political leverage of large commercial car manufacturers. Due to their size and scope, throughout the middle to late 20th century, commercial car manufacturers had a powerful lobby, particularly in their home market, to limit vehicle taxes, neglect public transport, and advocate for more road construction and parking in cities (Reich, 1989; Paterson, 2000; Dicken, 2011). One may wonder why we do not use, say, 1950 instead of 1920 as the reference year. When including car manufacturers from more recent years, the first-stage is likely stronger, but at the same time it is more likely that the instrument is correlated to omitted variables that are correlated with car ownership and population density. Hence, we prefer to use car manufacturers from earlier times.[163]

We emphasise here that we are not the first study to argue for the impact of the automotive industry on current car policy. Probably the most well-known example of how the automotive industry affected car policies is the so-called Streetcar Conspiracy, where General Motors and other car manufactures were convicted of monopolising the sale of buses and accused of controlling the transit system in order to dismantle existing streetcar networks, which were replaced by buses (Richmond, 1995). In Europe, Cleff et al. (2005) find that EU "member states having a large car industry tend not to apply a registration tax, or they apply a lower registration tax, while car importing member states tend to levy a higher registration tax". There is also anecdotal evidence that the automotive industry in France launched a powerful lobby in the 1950s against railways in order to promote road construction (Meunier, 2002).

Many large car manufacturers during the 1900s began in the early 20th century, but not all. Countries such as Japan and South Korea in Asia and Spain and Sweden in Europe also developed substantial capacity to manufacture cars, while car manufacturers also set up plants and subsidiaries in other countries. In both cases however, it is likely that the political influence was smaller than in the early car manufacturers because these industries were active for a shorter period of time and there is a tendency to support national industries as opposed to foreign subsidiaries. Never-

Firstly, manufacturing plants were large and employed many workers, therefore car manufacturers may have had a direct effect on urban structure at the local level. Secondly, in most countries, national governments determine levels of car and fuel taxation as well as the layout of highways, which are the mechanisms through which car ownership is likely higher.

[162] The US had 8,132,000 registered automobiles and a population of 106,461,000 in 1920 (US Census, 2000). Car ownership was much lower in 1910 and is estimated to be around 500,000. In Section 5.4.5 we exclude the US as a sensitivity check.

[163] We provide robustness of the results of the choice of reference year in Appendix 6.

theless, if these countries are also treated with lower vehicle costs it implies that our IV estimate remains unbiased, but our first-stage *F*-statistic will be weaker.

Countries with a historic car manufacturer after 1920 are therefore likely to have higher rates of car ownership, while the presence of a car manufacturer in 1920 is unlikely to be directly related to urban structure after 1950, other than via car ownership. We will also demonstrate that car manufacturers were not more likely to start up in countries with lower 1920 population density.[164] Furthermore, one may argue that our instrument is *conditionally valid* on the level of economic development in 1920, as more industrialized countries were likely to have more car manufacturers. GDP in 1920 may be correlated to current car ownership and urbanisation rates. We therefore also estimate specifications where we condition on GDP per capita and population at the country level in 1913.[165] We present further evidence, including several mechanisms, and plausibility of the instrument in Section 5.4.2.

5.4 Results

We first present OLS results of the relation between car ownership and population density (Section 5.4.1), then present evidence on the plausibility of our instrument (Section 5.4.2) and the IV results (Section 5.4.3). Finally, we discuss some extensions and perform a range of robustness checks (Section 5.4.5).

5.4.1 OLS Results

First we regress the log of population density on cars per capita, controlling only for decade fixed effects. There is a strong and statistically significant negative association. One additional car per 100 inhabitants is associated with a reduction in average population density of around 3%.[166] In column (2) we control for GDP per capita, but the effect of car ownership hardly changes.[167]

[164] Hence, we address the issue that residents of cities that were more dense before the introduction of the car may have adopted fewer cars and therefore remained more dense.

[165] The estimate is somewhat imprecise, but more importantly, the point estimate is essentially the same as in the preferred specification.

[166] This is calculated as $100 \cdot (\exp(\beta) - 1)$.

[167] Note that GDP per capita, or income, does not have a statistically significant effect on population density *when controlling for car ownership*. This result holds regardless of functional form (results from including GDP per capita linearly or with quadratic terms are available upon request). As we will see that income has a strong positive effect on car ownership, in line with Dargay (2002), the overall effect of income on population density appears to be via increased car ownership (see

Table 5.2: OLS estimates

	Dep var: Population density (log)				
	(1)	(2)	(3)	(4)	(5)
Cars per 100	-0.0307***	-0.0302***	-0.0243***	-0.0236***	-0.0218***
	(0.00490)	(0.00705)	(0.00541)	(0.00554)	(0.00620)
GDP per capita (log)		-0.0115	0.00803	-0.0373	-0.0346
		(0.0773)	(0.0564)	(0.0593)	(0.0695)
Pop dens. 1920 (log)			0.455***	0.426***	0.356***
			(0.0706)	(0.0705)	(0.0621)
January temperature (°C)				-0.00917	-0.00821
				(0.00665)	(0.00696)
July temperature (°C)				0.0118	0.00869
				(0.0125)	(0.00981)
Annual precipitation (m)				0.182	0.173
				(0.136)	(0.122)
Altitude (km)				-0.182*	-0.185**
				(0.103)	(0.0792)
Ruggedness				0.541**	0.292
				(0.247)	(0.198)
French legal origin					0.476***
					(0.148)
German legal origin					0.302**
					(0.151)
Scandanavian legal origin					-0.274
					(0.170)
Decade FE	Y	Y	Y	Y	Y
R^2	0.502	0.502	0.623	0.660	0.715
No. of countries	57	57	57	57	57
No. of cities	123	123	123	123	123
No. of observations	232	232	232	232	232

Notes: Estimates are weighted by the number of observations per city. Robust standard errors are in parenthesis and are clustered at the country level. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In columns (3)-(5), we include historical population density in 1920 and some additional geographical and legal origin controls. The coefficient of historic population density is positive and statistically significant with an elasticity of 0.46, indicating that density is persistent over time. Including 1920 population density reduces the coefficient of interest slightly to -2.4%, meanwhile there is no noticeable effect from

first-stage results in Table 5.4).

the inclusion of climate and terrain controls in column (4).[168] The effect size declines somewhat in column (5) when controls for legal origin are included. The results indicate that countries with French and German legal origins have higher urban densities than countries with English and Scandinavian legal origins. The effect of one additional car per 100 inhabitants is associated with a reduction in population density of around 2.2% and is still statistically significant at the 1% level.

Let's now consider Oster's (2019) bias-adjusted estimate. Under the recommended assumption of $\delta = 1$ and $R^2_{\max} = 1$, the bias corrected estimate is -0.043, with a 95% confidence interval between $[-0.060, -0.025]$.[169] We also loosen the assumptions on the δ parameter (see Figure 5.B.1 in Appendix 1). For $\delta \in [0.75, 2]$ we calculate the bounds as $[-0.045, -0.038]$ with an average estimated effect of -0.039.[170] This suggests that the OLS coefficient may be somewhat downward biased. We will see that bias-corrected estimates are also somewhat larger than the IV results in Section 5.4.3.

5.4.2 Car manufacturers in 1920

There is a priori no reason why city structure in 1920 was related to the presence of a car manufacturer in 1920 because few people owned cars. Meanwhile, over the subsequent decades, car manufacturers had substantial political leverage and had a strong lobby in their home market to increase car demand (Reich, 1989; Paterson, 2000; Dicken, 2011).

To investigate the latter, we will now examine how the presence of a historic car manufacturer is associated with lower generalised prices for car use at the end of the 20th century.[171] In 1920, nine countries had a domestic commercial car manufacturer.[172] In Table 5.3 we provide empirical evidence that these countries had lower ownership taxes and lower costs of car use in 1995, even when we control for GDP per capita and other controls.[173] Column (1) indicates that the monetary price of an average car trip, which includes both variable and fixed costs, in 1995 is about 30% lower in

[168] Climate, proxied by summer and winter temperatures, may be correlated to both driving and building types. Both the OLS results and the first-stage results (see Table 5.4) do not indicate this to be the case.

[169] Standard errors are cluster-bootstrapped (250 replications) based on countries.

[170] As δ increases, the causal estimate converges to around -0.037.

[171] The MCD 1995 dataset includes information on transport-related costs, however we also have information on road and highway length for a larger sample of cities and time periods.

[172] Australia, Canada, the Czech Republic, France, Germany, Italy, Russia, the United Kingdom, and the United States. See Appendix 3.

[173] We have fewer observations here than in the main analysis because of missing information.

Table 5.3: Underlying mechanism

	(1) Car trip cost	(2) Capital cost	(3) Road length	(4) Highway length
Car manufacturer 1920	-0.288*	-0.109	0.377*	0.248
	(0.163)	(0.146)	(0.215)	(0.226)
Controls	Y	Y	Y	Y
Decade FE	Y	Y	Y	Y
R^2	0.613	0.685	0.679	0.581
No. of countries	43	43	50	44
No. of cities	89	92	108	94
No. of observations	89	92	200	124

Notes: Dependent variables are in logs. See Table 5.A.3 in Appendix for descriptive statistics. Car trip cost is defined as the direct user cost of an average car trip and includes depreciation, fuel, spare parts, insurance and taxes. Capital cost is defined as the annual fixed costs which includes depreciation, insurance and taxes. Controls are the log of GDP per capita, 1920 population density, January and July temperature, annual precipitation, altitude, ruggedness, and legal origin fixed effects, as in column (5) of Table 5.2. Road and highway length are per capita. Robust standard errors are in parenthesis and are clustered at the country level. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

countries with historic car manufacturers. Furthermore, these countries have lower annual capital car costs (which include taxes, column (2)), more roads (column (3)), and more highway kilometres per capita (column (4)). However as these effects are imprecisely estimated, they should be interpreted with caution. In Appendix 3 we also document that in 2005, countries with a historical car manufacturer in Europe charge between 20–50% lower vehicle taxes overall, and face almost zero registration taxes, while fuel taxes are somewhat higher (Kunert and Kuhfeld, 2007).

We present the first-stage results of the IV estimates in Table 5.4. Columns (1) to (5) indicate that the presence of a commercial car manufacturer in 1920 is a strong instrument.[174] The Kleibergen-Paap first-stage *F*-statistic is 18.58 in the last (and preferred) specification. The instrument has the expected positive sign: countries with a commercial car manufacturer owned around 15 more cars per capita (or around 50% more than the mean city in our sample).

We come now back to our claim that a priori, there is no reason why population density in 1920 was related to the presence of a car manufacturer in 1920. In Table 5.5 we examine whether car manufacturers in 1920 were more likely to be present in cities with lower population densities, as arguably, the instrument is more convincing if it is not related to population density in 1920. Columns (1) and (2) indicate that the

[174] Note that car ownership at the city level strongly increases with GDP per capita, and falls with historic population density).

Table 5.4: First-stage results

	Dep var: Cars per 100				
	(1)	(2)	(3)	(4)	(5)
Car manufacturer 1920	27.95***	15.70***	15.94***	15.86***	14.73***
	(4.610)	(3.711)	(3.137)	(2.812)	(3.411)
GDP per capita (log)		9.318***	8.368***	8.244***	8.309***
		(0.747)	(0.729)	(1.059)	(1.081)
Pop dens. 1920 (log)			-4.406**	-4.913**	-4.524**
			(1.891)	(1.871)	(1.889)
Climate controls	N	N	N	Y	Y
Terrain controls	N	N	N	Y	Y
Legal origin FE	N	N	N	N	Y
Decade FE	Y	Y	Y	Y	Y
R^2	0.493	0.726	0.749	0.763	0.765
First-stage F-statistic	36.75	17.89	25.83	31.80	18.64
No. of countries	57	57	57	57	57
No. of cities	123	123	123	123	123
No. of observations	232	232	232	232	232

Notes: Estimates are weighted by the number of observations per city. Robust standard errors are in parenthesis and are clustered at the country level. See Table 5.B.1 in Appendix 6 for table including all controls. Climate controls are; January and July temperatures and annual precipitation, and terrain controls are; altitude and ruggedness as in column (5) of Table 5.2. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Kleibergen-Paap F-statistic is presented.

presence of a historic car manufacturer is not correlated to historic population density, independent of whether we include controls. If anything, the point estimate in the preferred specification in column (2) suggests that population density was higher in countries with a car manufacturer in 1920.[175]

We also estimate the reduced-form effect on current population density (after 1960) in columns (3) and (4). The results indicate that the presence of a car manufacturer in 1920 has a strong, negative, and statistically significant, effect on population density many years later.[176] Hence, historic car manufacturers are not related to historic population density, but have a causal effect on current density. In the following 2SLS analysis we control for historic population density, but we emphasise that the results of Table 5.5 imply that our instrument is also unconditionally valid, *i.e.* when not controlling for historic population density, we will obtain the same effect for car own-

[175] This is not surprising given that, for example, US cities faced higher urban densities until 1950, after which they began to decline (Kim, 2007).

[176] A formal test for the difference between the effect of car manufacturers in (2) and (4) rejects the null hypothesis that there is no difference at the 95% confidence level. The difference is -0.62, with a standard error of 0.26 and a corresponding t-statistic of -2.40.

Table 5.5: Additional validity tests of instrument

	Population density 1920 (log)		Population density (log)	
	(1)	(2)	(3)	(4)
Car manufacturer 1920	-0.227	0.249	-0.819***	-0.363**
	(0.270)	(0.206)	(0.289)	(0.158)
Decade FE	Y	Y	Y	Y
Controls	N	Y	N	Y
R^2	0.0520	0.281	0.232	0.661
No. of countries	57	57	57	57
No. of cities	123	123	123	123
No. of observations	232	232	232	232

Notes: Estimates are weighted by the number of observations per city. The dependent variable is historic (1920) or observed (1960-2012) population density in logs. Robust standard errors are in parenthesis and are clustered at the country level. Controls are the log of GDP per capita, January and July temperature, annual precipitation, altitude, ruggedness, legal origin fixed effects, and in columns (3) and (4) we also include 1920 population density, as in column (5) of Table 5.2. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

ership.

5.4.3 IV Results

In Table 5.6 we provide the 2SLS results using the presence of a historic car manufacturer as an instrument. The coefficient of interest remains quite stable to the inclusion of control variables and are of a similar order of magnitude to the OLS estimates. Furthermore, the control variables have a similar effect as compared to the OLS estimates in Table 5.2.

The preferred specification in column (5) indicates that one additional car per 100 inhabitants is associated with a reduction in population density of around 2.4%. A Hausman test does not reject the null hypothesis that the IV and OLS coefficients on cars per 100 in column (5) are significantly different from each other.[177] The IV estimate in column (5) is in between the OLS estimate in Table 5.2 and the bias-corrected estimate using Oster's (2019) method in Section 5.4.1. Apparently, the reverse causality issue is too small to have consequences for the estimates, at least at the city level.[178] The estimate implies that a one standard deviation increase in car ownership (20 cars

[177] We perform a cluster-robust Hausman test with 250 bootstrap replications (Cameron and Trivedi, 2005). The test statistic is $\chi^2(1) = 0.02$, with a corresponding p-value of 0.89.

[178] Duranton and Turner (2018) find that urban density has a small negative effect on vehicle kilometres driven, however their study is at the household level rather than the city level.

Table 5.6: 2SLS estimates

	Dep var: Population density (log)				
	(1)	(2)	(3)	(4)	(5)
Cars per 100	-0.0293***	-0.0268**	-0.0283***	-0.0261***	-0.0247**
	(0.00718)	(0.0133)	(0.00989)	(0.0100)	(0.0102)
GDP per capita (log)		-0.0542	0.0550	-0.0103	-0.00380
		(0.146)	(0.110)	(0.109)	(0.114)
Pop dens. 1920 (log)			0.439***	0.414***	0.346***
			(0.0726)	(0.0752)	(0.0693)
Climate controls	N	N	N	Y	Y
Terrain controls	N	N	N	Y	Y
Legal origin FE	N	N	N	N	Y
Decade FE	Y	Y	Y	Y	Y
First-stage F-statistic	36.75	17.89	25.83	31.80	18.64
No. of countries	57	57	57	57	57
No. of cities	123	123	123	123	123
No. of observations	232	232	232	232	232

Notes: Estimates are weighted by the number of observations per city. See Table 5.B.2 in Appendix 6 for table including all controls. Climate controls are; January and July temperatures and annual precipitation, and terrain controls are; altitude and ruggedness as in column (5) of Table 5.2. Robust standard errors are in parenthesis and are clustered at the country level. Kleibergen-Paap F-statistic is presented. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

per 100 inhabitants) is associated with a reduction in population density of around 40%.[179] We find a smaller effect than in Glaeser and Kahn (2004) who find an effect size around three times as large.[180]

5.4.4 Extensions

5.4.4.1 Other dependent variables

In Table 5.7 we present results separating population density into population and area size of the city, and consider two additional dependent variables: employment density and employment centrality, defined by the number of jobs in the CBD. Columns (1) and (2) indicate that although the effect of car ownership rates is not statistically significant when regressing both variables separately, the point estimate of cars per 100 on the log of built-up area is 0.025, which is essentially identical to the estimate

[179] This can be calculated as: $\exp(\hat{\beta} \times \Delta C) - 1 = \exp(-0.0245 \times 20) - 1 = -0.387$ or 39%.
[180] Here we refer to their coefficient in Table 6, column (3), which is -0.075 or 7.2%.

Table 5.7: 2SLS Sensitivity checks: Other dependent variables

	(1) Population	(2) Area	(3) Emp. density	(4) Prop. Jobs CBD
Cars per 100	0.000371	0.0250	-0.0216*	-0.0112
	(0.0158)	(0.0212)	(0.0111)	(0.0124)
Controls	Y	Y	Y	Y
Decade FE	Y	Y	Y	Y
First-stage F-statistic	18.64	18.64	14.53	17.54
No. of countries	57	57	53	46
No. of cities	123	123	112	112
No. of observations	232	232	144	93

Notes: Dependent variables are in logs. Estimates are weighted by the number of observations per city. Controls are the log of GDP per capita, 1920 population density, January and July temperature, annual precipitation, altitude, ruggedness, and legal origin fixed effects, as in column (5) of Table 5.2. Robust standard errors are in parenthesis and are clustered at the country level. Kleibergen-Paap F-statistic is presented. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

of cars on population density in column (5) of Table 5.6, meanwhile the point estimate on population is essentially zero. This is in line with the sprawl hypothesis as it suggests that the overall effect of cars on population density appears to be via cars causing cities to spread out further (Glaeser and Kahn, 2004; Nechyba and Walsh, 2004; Su and DeSalvo, 2008).

As mentioned in Section 5.2, population and employment density are highly correlated – therefore one expects similar effects for population and employment density.[181] Column (3) indicates that one additional car per 100 inhabitants causes a reduction in employment density of around 2.1%, but this effect is just not statistically significant at the 5% level, and only significant at the 10% level. We also examine whether car ownership rates reduce the proportion of jobs in the CBD in column (4), however we do not find a statistically significant effect.

5.4.4.2 Heterogeneous effects

We also investigate whether the effect of car ownership on population density varies by *(i)* infrastructure quality, *(ii)* level of GDP, and *(iii)* origin of law system. Table 5.8 reports the results.[182] Columns (1) and (2) suggest that roads and highways appear

[181] Baum-Snow (2010) also finds similar effects of highways on the decentralisation of firms and households.

[182] To improve efficiency of the estimates, we include interactions of the fitted values from the first-stage with road length, GDP per capita and legal origin. We refer to Levkovich et al. (2017) who

to have a complementary effect, so both roads *and* cars are important in facilitating urban decentralisation. Interestingly, roads also have a statistically significant and negative effect on population density, while the effect of highways is a lot smaller. This may be because road space occupies a large share of land, which can no longer be used for residential purposes.

In column (3) we find some evidence that the effect of car ownership on population density is larger in countries with a higher GDP. This result might be reasonable as richer countries are likely to invest in road infrastructure that strengthen the effect of car ownership.

Finally, column (4) indicates that while the effect of car ownership appears to be similar for countries with English, German, and Scandinavian legal origins, countries with French legal origins appear to have significantly smaller effects (around half). This indicates that countries with French legal origins are not only more regulated in terms of taxation (Glaeser and Kahn, 2004) but also have more planning restrictions.

5.4.5 Robustness

The IV results indicate that one additional car per 100 inhabitants reduces population density at the city level by 2.4% in the long-run. In this subsection we perform a wide range of robustness checks, and provide some tentative evidence on the middle-run effect.

5.4.5.1 Fixed-effects models

Up to now, we have exploited (mainly) cross-sectional variation in population density and car ownership. We assess the sensitivity of our results to various alternative types of variation, by gradually including a more detailed set of fixed effects. The first column of Table 5.9 shows IV results and columns (2)-(5) show OLS results. In column (1) and (2) we include continent fixed effects. The limitation of this analysis is that because our instrument varies at the country level, the degrees of freedom at the country level is strongly reduced. As a consequence, the first-stage F-statistic falls below 10 and the coefficient of interest becomes imprecise. Nevertheless, including continent fixed effects leads to similar (slightly larger) results than our main specifi-

compare different methodologies.

cation. The OLS regression with continent fixed effects in column (2) delivers a very similar estimate as compared to the baseline OLS estimate.

Next we include country fixed effects, therefore we only exploit cross-sectional variation *within* a country. This has the advantage that we can control for unobserved factors at the national level such as regulations that effect both vehicle ownership and urban density, however as our instrument does not vary at the city level, we are unable to correct for reverse causality. So far, our results have shown that the OLS and IV results are remarkably similar, and the OLS results are generally more conservative, therefore we tentatively perform this analysis to check the robustness of our results, but urge caution when interpreting the coefficients as causal effects. The results in column (3) indicate that the effect is roughly similar, one additional car is associated with a reduction in population density of around -1.5% and is statistically significant at the 10% level.

We then exploit temporal variation by including city fixed effects in column (4). This provides a tentative estimate of the middle-run effects. The estimate however becomes close to zero and is not statistically significant. An issue with this specification is that urban density changes slowly and information about cities that are observed with few years in between comes from different sources, implying substantial measurement error. The latter is problematic, because the downward bias due to measurement error is compounded with panel data. In order to overcome the inconsistency, Cameron and Trivedi (2005) recommend using longer differences, therefore in specification (5) we only select observations for cities that are at least 20 years apart. This leaves us with 77 cities and 29 observations. The coefficient is -1.5% and statistically significant at the 1% level. This is smaller in magnitude than our main IV result, but around the same size as the OLS result when including country fixed effects, suggesting that the middle-run effect may be about half of the size of the long-run effect.

5.4.5.2 Other robustness checks

In Appendix 3 we consider alternative proxies for automobile use. We include motorbike use as a separate variable and together with car ownership, leading to similar results. We also consider measuring car usage directly, by using car km per capita and car km per car. For car km per capita we find almost identical results. However, for car kilometres per car the results are imprecise because of a weak first stage. This may be because car manufacturers focus lobbying efforts on car ownership and purchase, while having smaller effects of car usage such as fuel taxes, which is corroborated by our analysis on European countries in Appendix 3. Still, despite weak instruments, we find a negative second-stage coefficient of car use on population density of the same order of magnitude as the baseline estimates.

A large literature has investigated the effects of highways on population density and decentralisation. In Appendix 4 we further investigate whether the effect of car ownership and infrastructure on population density are complementary. However, note that roads and highways cause cities to decentralise only when there is sufficient car use. We show that roads and highways have the expected negative effect on population density. The effect of car ownership is only reduced by about 20%, suggesting that we also expect decentralisation to happen when the infrastructure is still immature (say in cities in Sub-Saharan Africa).

Our data is composed of three main data sources, obtained from Ingram and Liu (1999), Kenworthy and Laube (2001), and UITP (2015). In Appendix 5 we assess the sensitivity of our results to the various data sources. There are two important observations from this exercise. The first-stage F-statistic becomes weaker for more recent data; and the second-stage estimate becomes lower. This suggests that car manufacturers were more powerful in the 1960s and 1970s in influencing policy, which is in line with anecdotal evidence. The reduction in the magnitude of the second-stage coefficient appears to be driven by the inclusion of other cities and countries. This suggests that the sample of cities in Ingram and Liu (1999) is not completely representative.

We consider various other robustness checks in Appendix 6. We first make sure that the effect of car manufacturers in 1920 on car ownership is not confounded by GDP per capita around that time, as car manufacturers in 1920 tended to be present in higher income countries. Although the coefficient of interest becomes somewhat imprecise, the magnitude of the point estimate increases slightly and is very close to our preferred baseline estimate.

One may be worried that our results are driven by cities in the US due to their firmly established car culture and the abundance of land for urban expansion. However, when we exclude the 12 cities in the US from our sample (27 observations) the effect size decreases only slightly to -2%. Furthermore, country borders changed over the twentieth century. This may affect assignment into treatment as these countries were possibly treated at some point in time, but would be mislabelled as non-car manufacturing. Therefore we exclude countries that were formerly part of the Soviet block. This has no meaningful impact on the second-stage results, while the first-stage F-statistic increases.

As an instrument we use a dummy whether there is a car manufacturer in 1920. Alternatively, we also consider to use a dummy whether there is a car manufacturer in 1910 or 1930. This leads to similar results, although the results are statistically stronger once we use the dummy indicating whether there is a car manufacturer in 1930.

5.4.6 Implications

Overall, our preferred estimate from column (5) in Table 5.6 implies that an increase in car ownership of one car per 100 inhabitants leads to a reduction in population density of around 2.4%. In this section, we apply this estimate to gauge the potential effects of growing car ownership rates in developing countries and the introduction of autonomous vehicles on urban density.

5.4.6.1 Growing car ownership in developing countries

In 1995, cities located in developing Asian countries owned substantially fewer cars per capita and faced higher population densities (see Table 5.A.4 in Appendix). Applying our estimates suggests that if car ownership increases to similar rates as seen in western Europe, urban density would fall by around 50% in the long-run, while if car ownership rates reach levels seen in North America and Oceania, density would even fall by around 60%.

We have applied our estimates for three Chinese cities in our dataset: Beijing, Guangzhou, and Shanghai. In 1995 average car ownership in these cities was 2.6 cars per 100 inhabitants which grew to 17.5 by 2010.[183] According to our results this would result in a reduction in population density of around 30%, whereas the actual reduction was around 60% (it declined from $14,600$ to $5,600$ people per km^2). This suggests that changes in car ownership explain about half the reduction in population density.[184]

5.4.6.2 Automated vehicles

Our estimates are also relevant in the broader context of future transport developments such as fully automated vehicles (AVs) which are expected to increase access

[183] Information for the year 2010 is gathered from ITF (2017) and Demographia (2010).

[184] As the period between 1995 and 2010 is relatively short, it is plausible that the contribution of car ownership is less.

to cars (Meyer et al., 2017; Gelauff et al., 2019). These results are particularly relevant to cities with relatively high incomes, but low levels of car ownership such as Copenhagen (Denmark) and Tokyo (Japan). Currently, car use is limited by ownership, however, AVs are expected to reduce the fixed costs of owning a car and thereby may substantially increase vehicle access. In the absence of policy, our results suggest that cities are expected to become more decentralised.

Fagnant and Kockelman (2015) assume that AVs are expected to increase vehicle kilometres travelled (VKT) by $10 - 20\%$ in the US. We use our estimate for the change in VKT per capita in column (4) of Table 5.B.3 in Appendix 3 and consider these changes as lower and upper bounds of the effects of AVs. In scenario (A) effective car ownership for an average city in our dataset increases by 10%, or 420 km per person, leading to a decline in population density of around 6.5%. In scenario (B) we consider a more extreme situation where equivalent car use increases by 20%, which is expected to result in population density declining by around 12.5% in the long-run.

While these estimates provide a rough indication of the potential effects of AVs, there may be reasons to expect that they may be over- or under-estimates. On the one hand, because AVs can be shared and therefore do not require car ownership, this this may free up vast amounts of parking space in inner cities which could be used for residential and other purposes, while on the other hand, because commuters can engage in other activities in the vehicle, such as sleeping or working, this might lead to longer commutes than currently tolerated (Pudāne et al., 2019).

5.5 Conclusion

Cars have dominated the urban landscape over the past century. In this paper we investigate the long-run impact of car ownership on urban form, in particular on population density, in an international sample of cities. Using the presence of a car manufacturer in 1920 as a source of exogenous long-term variation in vehicle costs, our IV estimates indicate that higher car ownership rates, induced via lower ownership costs, substantially reduce densities. A one standard deviation increase in car ownership rates (or 20 cars per 100 inhabitants) causes a reduction in density of around 40% in the long-run. Disentangling this effect between population and city size suggests that the major driver of this reduction in urban density is via the outward expansion of the city as the size of urban areas increase. Furthermore, we find that the effects are larger in cities with more roads and highways, and a higher income; they are lower in countries with French legal origins, which may have stricter vehicle taxation and land use regulations.

Our findings suggest that unpriced market failures in the car market have additional spillovers on urban density. This has implications for the key benefits of living and working in a city, and may justify higher taxes on private vehicle ownership and use in order to increase the benefits associated with higher densities, such as positive agglomeration economies and public transport efficiency, and decrease the costs associated with lower densities, such as pollution and environmental damage. Furthermore, the paper also has implications for expected urban growth in developing countries, where car ownership rates and populations are rapidly increasing, and future transport technologies such as automated vehicles, which are expected to dramatically reduce the costs of using a private vehicle.

Table 5.8: 2SLS sensitivity checks: Heterogeneity

	Dep var: Population density (log)			
	(1)	(2)	(3)	(4)
	Road leng.	Highway leng.	GDP	Legal origin
Cars per 100 (demean)	-0.0178**	-0.0146	-0.0176	-0.0330***
	(0.00859)	(0.0106)	(0.0113)	(0.00921)
× Road length demean (log)	-0.00781***			
	(0.00175)			
× Highway length demean (log)		-0.00772***		
		(0.00235)		
× GDP per capita demean (log)			-0.00438**	
			(0.00215)	
× French origin				0.0226***
				(0.00449)
× German origin				0.00218
				(0.00675)
× Scandanavian origin				-0.0111
				(0.0131)
GDP per capita demean (log)			-0.140	
			(0.149)	
Road length demean (log)	-0.111***			
	(0.0397)			
Highway length demean (log)		-0.0460		
		(0.0553)		
GDP per capita (log)	-0.118	-0.176		-0.00991
	(0.118)	(0.150)		(0.108)
French legal origin	0.333**	0.475***	0.477***	0.493***
	(0.156)	(0.174)	(0.153)	(0.131)
German legal origin	0.123	0.235	0.316	0.229
	(0.204)	(0.211)	(0.200)	(0.185)
Scandanavian legal origin	-0.265	-0.299	-0.178	-0.264
	(0.227)	(0.308)	(0.266)	(0.219)
Controls	Y	Y	Y	Y
Decade FE	Y	Y	Y	Y
First-stage F-statistic	18.64	18.64	18.64	18.64
No. of countries	50	44	57	57
No. of cities	200	124	232	232

Notes: Estimates are weighted by the number of observations per city. Controls are 1920 population density, January and July temperature, annual precipitation, altitude, ruggedness, and legal origin fixed effects, as in column (5) of Table 5.2. Robust standard errors are in parenthesis and are clustered at the country level. Kleibergen-Paap F-statistic is presented. Statistical significance is denoted as: * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 5.9: Additional sensitivity checks: fixed effects models

	Dep var: Population density (log)				
	(1)	(2)	(3)	(4)	(5)
	IV	OLS	OLS	OLS	OLS
Cars per 100	-0.0303	-0.0198**	-0.0145*	-0.00374	-0.0156***
	(0.0309)	(0.00840)	(0.00811)	(0.00434)	(0.00488)
Controls	Y	Y	Y	Y	Y
Decade FE	Y	Y	Y	Y	Y
Area FE	Continent (6)	Continent (6)	Country	City	City
R^2		0.742	0.888	0.964	0.967
First-stage F-statistic	3.895				
No. of countries	57	57	57	34	15
No. of cities	107	107	107	60	29
No. of observations	232	232	232	169	77

Notes: Estimates are weighted by the number of observations per city. Controls are the log of GDP per capita, 1920 population density, January and July temperature, annual precipitation, altitude, ruggedness, and legal origin fixed effects, as in column (5) of Table 5.2. Robust standard errors are in parenthesis and are clustered at the country level. Kleibergen-Paap F-statistic is presented. Statistical significance is denoted as: * p < 0.1, ** p < 0.05, *** p < 0.01.

Appendix 5.A Additional descriptives

5.A.1 Appending data sources

Our main datasources are from Ingram and Liu (1999), Kenworthy and Laube (2001), and UITP (2015). Several important aspects should be noted. As some data points were missing in the original data, we imputed these observations using the most reliable data available.

5.A.1.1 Population density missing

The MCD 2012 dataset (UITP, 2015) was missing population density information for Addis Ababa (Ethiopia), Dublin (Ireland), Glasgow (Scotland), Izmir (Turkey), Johannesburg and Tshwane (South Africa), Mumbai (India), Nairobi (Kenya), Tehran (Iran), and Tokyo (Japan). Therefore, we collect data on the average population density of the built-up area in the closest available year (2015) from Smith (2017).[185]

Additionally, the MCD 1995 data (Kenworthy and Laube, 2001) did not include data for the total urbanised area in 1995 for Istanbul (Turkey), Lisbon (Portugal), Salvador (Brasil) and Turin (Italy). Therefore, we impute the data using two methods. Firstly, if there are other cities from the same country and year in the dataset, we take an average of the ratio between total surface area to urbanised area in the metropolitan region from the observable cities and use this to calculate the urbanised area in the missing city (the data always includes information on total surface area). If this was not possible, we used the urbanised area derived from 2002-2003 MODIS satellite data at 1 km resolution available from Schneider et al. (2003) for a given metropolitan area.

5.A.1.2 GDP per capita missing

The MCD 2012 dataset (UITP, 2015) is also missing GDP per capita for several cities, including; Addis Ababa (Ethiopia), Chicago, Portland OR (United States), Glasgow (Scotland), Helsinki (Finland), Izmir and Kocaeli (Turkey), Jerusalem (Israel), Johannesburg and Tshwane (South Africa), Mumbai (India), Nairobi (Kenya), Mashhad and Tehran (Iran), Melbourne (Australia), Montreal and Vancouver (Canada), Seoul

[185] An interactive chart is available at: `http://luminocity3d.org/WorldPopDen/#9/-26.2047/27.9987`.

(South Korea), Taipei (Taiwan), and Tokyo (Japan). We replace these missing data points using the most reliable online sources.

Furthermore, Caracas (Venezuela), Moscow (Russia), New Delhi (India) and Santiago (Chile) had no GDP data for 1995. For these cities we fill in the country level GDP per capita (in 1995 current USD) from the World Bank national accounts data.

5.A.1.3 Historical GDP and population 1913 missing

Historical data in 1913 is missing for Russia, Czech Republic, South Africa, Cote d'Ivoire, Israel, Senegal and Zimbabwe. For Russia and Czech Republic, we use data in 1913 from the Former USSR and Czechoslovakia. For South Africa, GDP data is taken from the closest year to 1913 which is 1910. For all other countries, except Israel, we back extrapolate the real GDP per capita in 1913 by calculating the average growth rate over the 20 year period 1950 - 1970. Using these growth rates we calculate a rough estimate for 1913. For Israel, it is less convincing to back extrapolate as the countries growth was substantially different after 1950 as Israel did not exist before 1948. Therefore we take an average of the neighbouring states in 1913, including Egypt, Syria, Palestine and Jordan. Finally, we collect historical population data at the country level from Lahmeyer (2006).

5.A.2 Historical population density

We use two main datasets to calculate a proxy of population density in 1920. We collect historical data on population size and the built-up area from the HYDE3.1 dataset and the spatial extent of urban areas in 2000 from sattelite images provided by Landscan (see Goldewijk et al. (2017) and Dobson et al. (2000), respectively, for the methods used). We perform the following steps:

1. Determine the urban spatial extent of metropolitan areas in 2000, the closest year we have global satellite data from Dobson et al. (2000) using methods from the Landscan (Patterson and Kelso, 2012).

    a) Using the Landscan dataset, we reclassify areas into urban if population density ≥ 200 pop/km^2.

    b) We then apply focal statistics to remove highways which are classified as cells with a height and width ≤ 2.

c) In the resulting focal statistics raster, cells having a (height and width) value ≥ 3 are considered urban so we assign value 0 to every cell with value < 3 and 1 for very cell with value ≥ 3.

d) Convert raster to polygons based on cells with values 1, which results in polygons of the urban areas.

2. Overlay the spatial extent polygons in 2000 on the HYDE data from 1920 and extract the sum of the population and built-up area in 1920.

3. Divide the 1920 population by the total built-up area in 1920 within the metropolitan boundaries of a city in 2000 to obtain population density in 1920.[186]

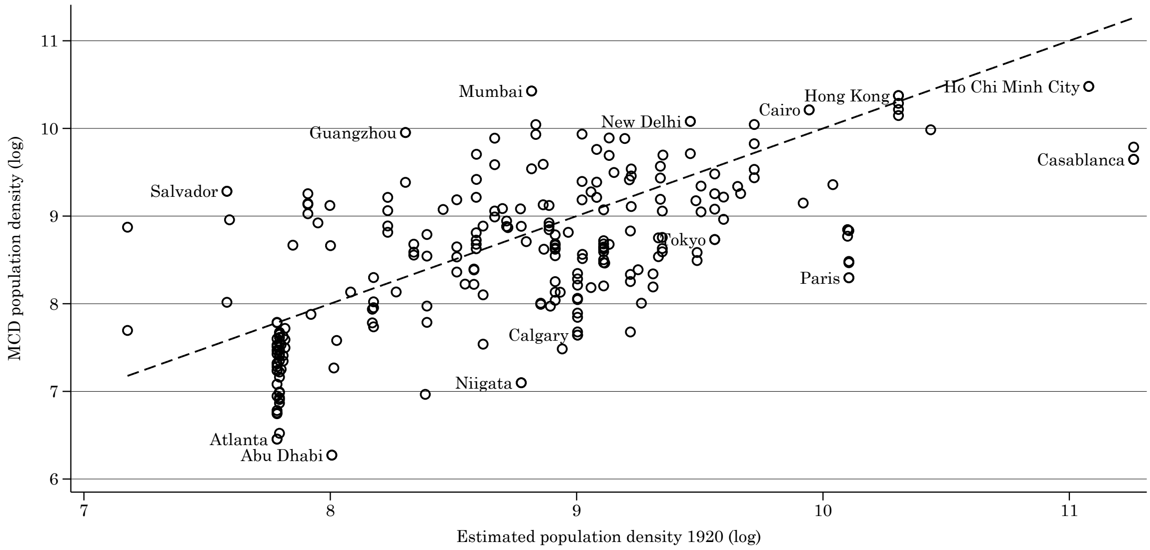
There are three limitations with the proposed estimate. Firstly, estimates of population density in 1920 are related to the urban spatial structure in 2000, and therefore may capture some of the effect of transport technologies over the past century. For our estimates this will mean that they are downward biased as the variable may capture some of the effect of interest and it will increase the likelihood of finding that car manufacturers were present in cities with lower densities. Note however that we do not find this (see Section 5.4.2).

Secondly, in order to estimate population densities in the past, Goldewijk et al. (2017) require assumptions on the dynamics of population density. Using the best cross-country data available, the authors find that population density at the city level initially increases until a certain point and then decreases, similar to the findings by Kim (2007) for US cities. The authors argue that this relation can be characterised by an asymmetric bell-shaped distribution. For each country, the size and the shape of the curve differ depending on the development stage in time. While we think this is a plausible assumption, as the distribution is fitted using few data points, it may not represent the true development pattern of cities in the past and especially may be poorly suited to represent cities outside Europe and North America where most historical city level data is available.

Finally, the historical estimation procedure assumes that all cities within a country develop in the same manner according to the country level distribution, therefore the

[186]For three cities we were unable to compute a population density measure because the polygon did not correctly overlay the raster file of population and built-up area (Dakar and Wellington) and because the city did not exist in 1920 (Brasilia). In the case of Dakar and Wellington, we selected the grid cell adjacent to the polygon area. For Brasilia, we took the average city density of the other Brasilian cities in our dataset.

Figure 5.A.1: Comparison of population density in data to 1920 estimate



Notes: This figure compares data from our compiled dataset which ranges between 1960 and 2012 to estimates from 1920 based on the method outlined above. The dotted line represents the 45 degree line of equality. City labels are based on minimum and maximum population densities for each 0.5 bin of estimated population density 1920 (log).

estimates are not able to capture the potential diversity in city developments over the past century and should be interpreted as an average city in a country. As our instrument is at the country level, we are interested in whether car manufacturers where more likely to be present in countries with lower city population densities, so a country level average is sufficient.

We perform various tests to confirm the reliability of the 1920 population density estimates. Firstly, a correlation of population density from the MCD in 1995 and our estimate using the method above for the year 2000 in logs is 0.84. This indicates a high correlation, and implies that our method of constructing population density seems valid. Secondly, Figure 5.A.1 illustrates the 1995 population density measures from MCD as compared to the estimates from 1920. The correlation in logs is 0.64 showing that density is persistent over time (Angel et al., 2010). It appears that density fell in 63% of cities between 1920 and our observatiton in the dataset which ranged between 1960 and 2012, declining on average from 8500 to 7800 people per km^2. Kim (2007) finds that average densities of US cities rose between 1890 and 1950 and fell between 1950 and 2000, however declined on average over the entire period. This is in line with Figure 5.A.1, as the majority of cities above the line of equality are in low and middle income countries where we may have expected densities to increase since the early 20th century, while cities below the line are generally in high-income countries where average densities generally fell.

Overall, it is plausible that the constructed measure captures the variation in population densities in urban areas between countries over the period, which is what we aim to measure in order to test the plausibility of our proposed instrument.

Table 5.A.1: Commercial car manufacturers in the early 20th century

Country	Manufacturer	Founded	Closed	Source on establishment
Australia	Holden	1914		https://bit.ly/34DELMS
Canada	McLaughlin Motor Car Company	1907	2018	https://bit.ly/3kILU4q
Czech Republic	Skoda	1895		https://bit.ly/37RXTsH
Czech Republic	Praga	1907		https://bit.ly/3jF2ssD
France	Peugeot	1889		https://bit.ly/2TyTsKO
France	Renault	1898		https://bit.ly/3jFrCrd
Germany	Audi	1910		https://bit.ly/31WYaa6
Germany	Benz	1885		https://bit.ly/3kLyswI
Germany	Opel	1899		https://bit.ly/37R56Jr
Italy	Fiat	1899		https://bit.ly/2HNZhBr
Italy	Alfa Romeo	1910		https://www.alfaromeousa.com/a-story-that-made-history
Japan	Isuzu	1922		https://bit.ly/2TD2cQl
Russia	Russo-Balt	1909	1918	Russian Motor Vehicles: The Czarist Period 1784 to 1917 by Maurice A. Kelly
Russia	Moskvitch	1929	2006	Cars of the Soviet Union: The Definitive History by Andy Thomson
Russia	NAMI	1927		Cars of the Soviet Union: The Definitive History by Andy Thomson
Sweden	Volvo	1927		https://volvocars.us/34BJZsK
United Kingdom	Morris	1913	1983	https://bit.ly/31Weaca
United Kingdom	Rover	1904	2005	https://bit.ly/3oLD8VN
United States	Ford	1903		https://bit.ly/35JqNIW
United States	Chevrolet	1911		Chevrolet: A History from 1911 by Beverly Rae Kimes, Robert C. Ackerson

5.A.3 Car manufacturers in 1920

In Table 5.3 we show evidence that cities in our dataset from countries with a car manufacturer in 1920 had lower vehicle costs and more roads per capita. Kunert and Kuhfeld (2007) collect regulatory charges for a representative European car, the Golf 1.4 with petrol engine in 2005. Table 5.A.2 provides additional descriptives which indicate that, even in 2005, countries in the European Union with a historical car manufacturer, still faced far lower levels of registration taxes while having higher taxes on petroleum.

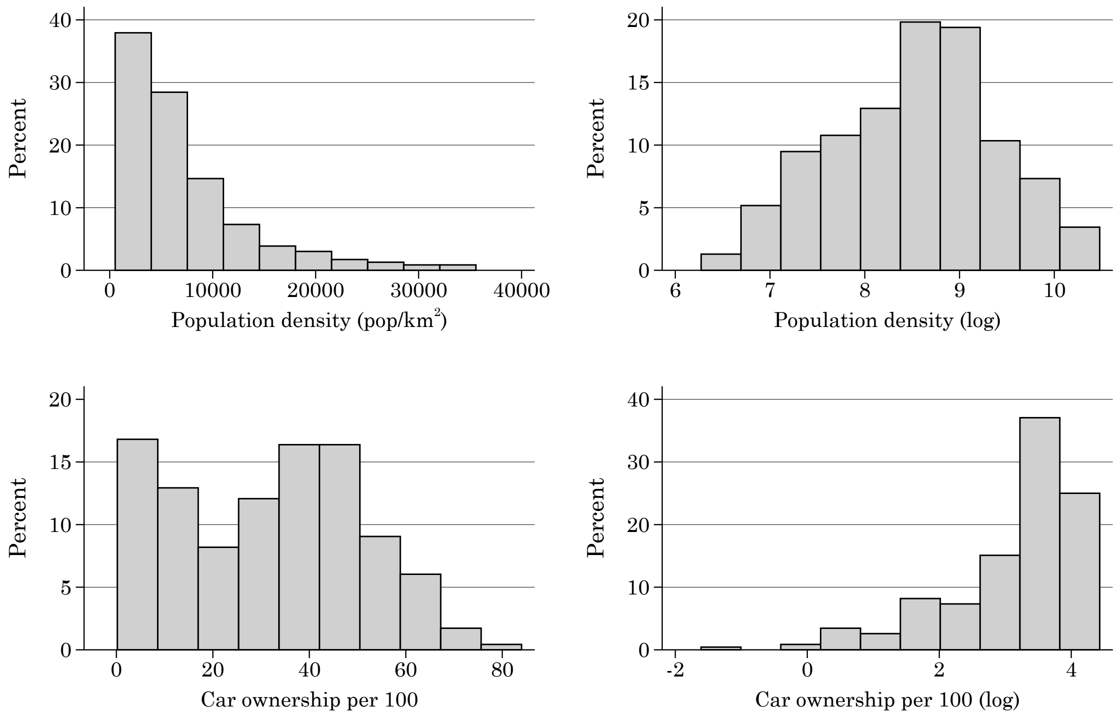
For petrol cars, total charges were 20% lower in countries with a historical car manufacturing country and registration taxes were almost zero as compared to an average of around €400 in non car manufacturing countries. Meanwhile, fuel taxes were approximately 25% higher although there is little difference in VAT on fuel. We see a similar albeit larger difference for diesel cars with total charges around 50% lower in countries with a historical car manufacturer. This provides additional evidence that car manufacturers lobbied particularly to keep costs of purchase low and encourage car ownership in their home countries, while we do not find evidence that in 2005, usage costs were particularly targeted.

Table 5.A.2: Additional evidence on mechanism: Private car charges in European countries, 2005

	Car manufacturer		Non car manufacturer		
	Mean	Std. dev	Mean	Std. dev	Difference (%)
Total charges petrol engine (€)	1266.00	217.63	1518.18	783.95	-19.92
Registration charges	29.00	28.61	384.68	542.02	-1226.49
VAT	326.80	28.12	345.77	117.16	-5.81
Vehicle tax	83.60	82.43	142.50	124.39	-70.45
Charges insurance	43.40	29.12	23.05	30.94	46.90
Petroleum tax	590.60	113.88	439.05	113.47	25.66
VAT on pretrolium	192.60	20.43	182.82	54.31	5.08
Total charges diesel engine (€)	1042.20	207.03	1563.00	931.79	-49.97
Registration charges	29.00	28.61	555.05	689.24	-1813.95
VAT	326.80	28.12	359.14	155.66	-9.89
Vehicle tax	122.60	127.78	238.95	221.64	-94.91
Charges insurance	53.80	36.13	28.59	38.26	46.86
Petroleum tax	369.40	105.84	252.77	50.98	31.57
VAT on pretrolium	140.40	16.68	128.55	32.97	8.44
GDP per capita indices	102.60	19.27	96.45	42.37	5.99

Notes: Data is collected from Kunert and Kuhfeld (2007). Regulatory charges are calculated for a representative European car, the Golf 1.4 with petrol engine and Golf 2.0 SDI with diesel engine in 2005. European countries with a car manufacturers in 1920 include: the Czech Republic, France, Germany, Italy, and the United Kingdom.

Figure 5.A.2: Histogram of key variables



5.A.4 Additional descriptives

In Figure 5.A.2 we report histograms of the key variables of interest. We observe that population density is approximately normally distributed when taking the log. However, it makes more sense to take car ownership in levels (as we do in the analysis); otherwise logged car ownership is strongly left-skewed.

In Table 5.A.3 we report additional descriptives for cost of car trips, annual capital costs for owning a car and length of roads and highways per capita, which we all use in Table 5.3. Because the cost of car trips and annual capital costs is only available in the MCD1995 data, we have fewer observations for these data.

In Table 5.A.4 we provide descriptive statistics by region and income level for population density and car ownership. Unsurprisingly, car ownership is positively correlated to the level of economic prosperity, with North America, Oceania and Europe having the highest car ownership levels. Cities in Asia have, by far, the highest population density.

Table 5.A.3: Additional descriptive statistics

	N	Mean	Std. dev	Min	Max
Cost of car trip	89	3.07	1.63	0.13	9.33
Annual capital car cost	92	2628.06	1517.25	152.32	10159.36
Road length per capita (m/pop)	200	3.47	3.28	0.15	17.21
Highway length per capita (m/pop)	131	0.12	0.21	0.00	1.63

Table 5.A.4: Main variables by region and income level in 1995

		Population density (pop/km^2)		Cars per 100	
Region	N. cities	Mean	Std. Dev.	Mean	Std. Dev.
Africa	5	5901	3058	11.45	10.22
Asia (high income)	6	15032	10126	21.03	11.78
Asia (low/middle income)	12	18639	8774	8.11	8.18
Eastern Europe	5	7136	4204	30.60	11.38
Latin America	10	9211	3634	18.74	7.86
Middle East	7	11657	7741	13.51	7.32
North America	15	1867	752	56.79	9.36
Oceania	5	1502	529	57.54	6.14
Western Europe	35	5483	2872	41.19	10.09

Note: Calculated based on our data.

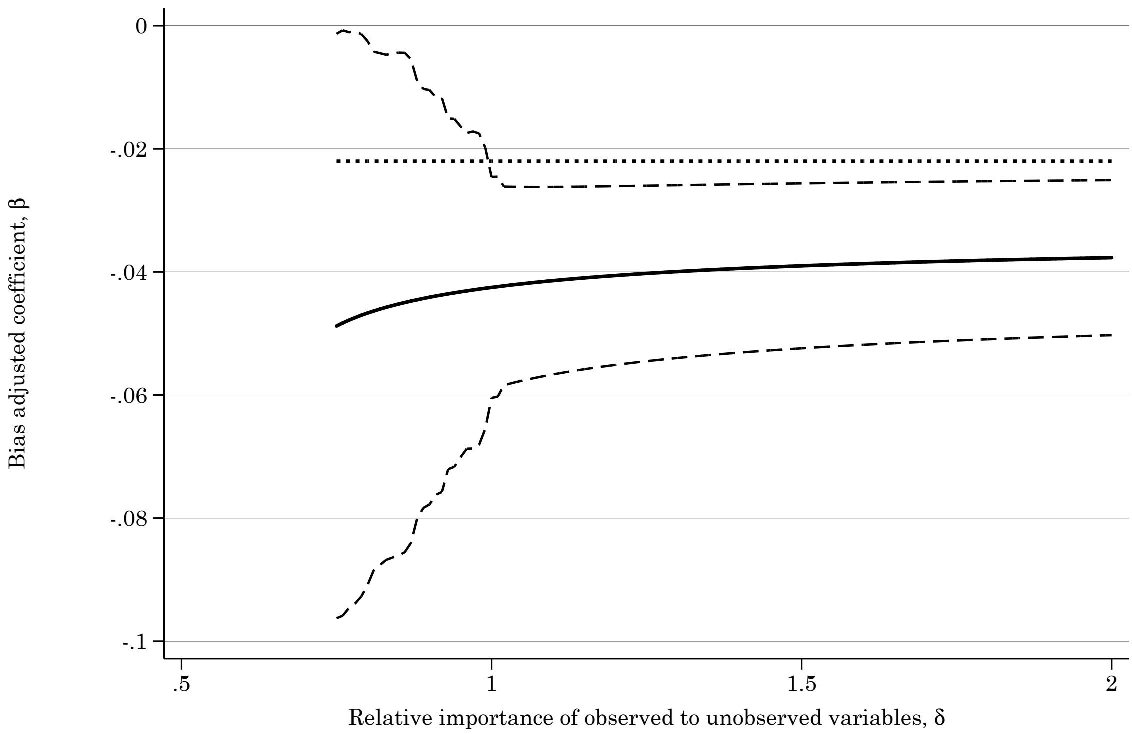
Appendix 5.B Additional results

5.B.1 Oster's bias-adjusted estimates

Here we report the results of Oster's (2019) bias-adjusted estimates for different values of δ. We set $R^2_{\max}$ to 1. Recall that δ depicts the relative degree of selection on observed and unobserved variables and $R^2_{\max}$ denotes the hypothetical R^2 resulting from a regression of population density on all observable and all unobservable variables. Because of measurement error, $R^2_{\max}$ is likely lower than 1 in most empirical application (Oster, 2019). We use cluster-bootstrapped standard errors (250 replications) based on countries to construct 95% confidence bands.

Figure 5.B.1 shows that the bias-adjusted estimate is always more negative as compared to the baseline OLS estimate. This suggests that the OLS coefficient may be biased downwards and provides a conservative estimate.

Figure 5.B.1: Oster's (2019) bias-adjusted estimator



Note: The solid line represents the bias-adjusted estimates and the dashed lines represent the 95% confidence interval where standard errors are cluster-bootstrapped (250 replications) based on countries. The short dotted line represents the OLS estimate in column (5) of Table 5.2.

Table 5.B.1: First-stage results

	Dep var: Cars per 100				
	(1)	(2)	(3)	(4)	(5)
Car manufacturer 1920	27.95***	15.70***	15.94***	15.86***	14.73***
	(4.610)	(3.711)	(3.137)	(2.812)	(3.411)
GDP per capita (log)		9.318***	8.368***	8.244***	8.309***
		(0.747)	(0.729)	(1.059)	(1.081)
Pop dens. 1920 (log)			-4.406**	-4.913**	-4.524**
			(1.891)	(1.871)	(1.889)
January temperature (°C)				-0.0784	-0.117
				(0.121)	(0.132)
July temperature (°C)				0.167	0.147
				(0.175)	(0.185)
Annual precipitation (m)				-2.005	-1.875
				(2.218)	(2.095)
Altitude (km)				2.405	2.128
				(1.929)	(1.992)
Ruggedness				4.639	5.517
				(4.953)	(4.933)
French legal origin					-2.364
					(2.960)
German legal origin					-2.236
					(4.112)
Scandanavian legal origin					-2.837
					(5.104)
Decade FE	Y	Y	Y	Y	Y
R^2	0.493	0.726	0.749	0.763	0.765
First-stage F-statistic	36.75	17.89	25.83	31.80	18.64
No. of countries	57	57	57	57	57
No. of cities	123	123	123	123	123
No. of observations	232	232	232	232	232

Notes: Estimates are weighted by the number of observations per city. Robust standard errors are in parenthesis and are clustered at the country level. Statistical significance is denoted as: * p < 0.1, ** p < 0.05, *** p < 0.01. Kleibergen-Paap F-statistic is presented.

5.B.2 Main tables with all controls

In Table 5.B.1 and 5.B.2 we present first-stage results and IV estimates, respectively, while showing the coefficients of the control variables. As can be seen, the controls have a similar effect as in the OLS specification in Table 5.2.

Table 5.B.2: 2SLS estimates

	Dep var: Population density (log)				
	(1)	(2)	(3)	(4)	(5)
Cars per 100	-0.0293***	-0.0268**	-0.0283***	-0.0261***	-0.0247**
	(0.00718)	(0.0133)	(0.00989)	(0.0100)	(0.0102)
GDP per capita (log)		-0.0542	0.0550	-0.0103	-0.00380
		(0.146)	(0.110)	(0.109)	(0.114)
Pop dens. 1920 (log)			0.439***	0.414***	0.346***
			(0.0726)	(0.0752)	(0.0693)
January temperature (°C)				-0.00955	-0.00929
				(0.00623)	(0.00690)
July temperature (°C)				0.0119	0.00848
				(0.0118)	(0.00919)
Annual precipitation (m)				0.174	0.169
				(0.129)	(0.114)
Altitude (km)				-0.178*	-0.184**
				(0.101)	(0.0766)
Ruggedness				0.551**	0.307
				(0.240)	(0.195)
French legal origin					0.460***
					(0.150)
German legal origin					0.273**
					(0.138)
Scandanavian legal origin					-0.325
					(0.215)
Decade FE	Y	Y	Y	Y	Y
First-stage F-statistic	36.75	17.89	25.83	31.80	18.64
No. of countries	57	57	57	57	57
No. of cities	123	123	123	123	123
No. of observations	232	232	232	232	232

Notes: Estimates are weighted by the number of observations per city. Robust standard errors are in parenthesis and are clustered at the country level. Kleibergen-Paap F-statistic is presented. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.B.3 Alternative proxies for automobile use

In Table 5.B.3 we assess the stability of our main results to various alternative measures of private vehicle usage. In column (1) we replace cars per 100 with private vehicles per 100, which therefore also includes motorbikes which are more prominent in low and middle income countries. The first-stage F-statistic is slightly lower at 14.12 (the first-stage results are reported in Table 5.B.4). One additional vehicle per 100 inhabitants results in 2.7% lower density, which is slightly larger than our main

Table 5.B.3: Total vehicles and car km per capita

	Dep var: Population density (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Vehicles per 100	-0.0277**					
	(0.0113)					
Cars per 100		-0.0287***				
		(0.00991)				
Motorbikes per 100		0.0265***				
		(0.00736)				
Car km p. cap. (100 km)			-0.0204***	-0.0159***		
			(0.00338)	(0.00495)		
Car km p. car (100 km)					-0.0338*	-0.0278
					(0.0198)	(0.0208)
Controls	Y	Y	N	Y	N	Y
Decade FE	Y	Y	Y	Y	Y	Y
First-stage F-statistic	14.22	21.31	8.215	12.49	1.565	1.173
No. of countries	55	55	51	51	51	51
No. of cities	123	123	107	107	107	107
No. of observations	222	222	123	123	123	123

Notes: Estimates are weighted by the number of observations per city. Controls are the log of GDP per capita, 1920 population density, January and July temperature, annual precipitation, altitude, ruggedness, and legal origin fixed effects, as in column (5) of Table 5.2. Robust standard errors are in parenthesis and are clustered at the country level. Kleibergen-Paap F-statistic is presented. Statistical significance is denoted as: * p < 0.1, ** p < 0.05, *** p < 0.01.

result. In column (2) we split cars and motorbikes per capita and only instrument cars per 100. Although it is unlikely that the coefficient on motorbikes per 100 inhabitants represents a causal effect, for example because scooters are a more attractive mode of transport in dense urban areas such as Rome and Amsterdam, the estimates in (1) and (2) along with the first-stage results in Table 5.B.4 suggest a positive correlation between motorbikes per capita and population density and a (weakly) positive correlation to car ownership. This likely explains why the coefficient of interest becomes slightly larger.

The final four columns of Table 5.B.3 regress the log of population density on the number of car km per capita and car km per car. The results in column (4), including all control variables from our preferred specification, indicates that the 2SLS estimates are statistically significant at the 1% level, although our instrument is less powerful (the first-stage F-statistic is 10). The coefficient suggests that a one standard deviation (3534 km) increase in car km per capita causes population density to decline by around 38% in the long-run, which is essentially identical to the results from our

Table 5.B.4: Total vehicles and car km per capita (First-stage results)

	Vehicles/Cars per 100		Cars km per capita/car (100 km)			
	(1)	(2)	(3)	(4)	(5)	(6)
Car manufacturer 1920	15.02***	14.66***	44.75***	28.70***	26.99	16.18
	(3.984)	(3.176)	(15.61)	(8.122)	(21.58)	(14.94)
Motorbikes per 100		0.260				
		(0.185)				
Controls	Y	Y	N	Y	N	Y
Decade FE	Y	Y	Y	Y	Y	Y
R^2	0.707	0.779	0.365	0.726	0.0847	0.340
First-stage F-statistic	14.22	21.31	8.215	12.49	1.565	1.173
No. of countries	55	55	51	51	51	51
No. of cities	123	123	107	107	107	107
No. of observations	222	222	123	123	123	123

Notes: Estimates are weighted by the number of observations per city. Robust standard errors are in parenthesis and are clustered at the country level. Controls are the log of GDP per capita, 1920 population density, January and July temperature, annual precipitation, altitude, ruggedness, and legal origin fixed effects, as in column (5) of Table 5.2. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Kleibergen-Paap F-statistic is presented.

main specification with car ownership rates as the main variable of interest. Finally, in columns (5) and (6) we estimate the effect of car km per car, however find that our instrument performs poorly and hence our 2SLS results are not very trustworthy. This may be because car manufacturers focus lobbying efforts on car ownership and purchase, while having smaller effects of car usage such as fuel taxes. Still, despite weak instruments, we find a negative second-stage coefficient of car use on population density.

5.B.4 Roads or cars?

The earlier literature emphasises the importance of road infrastructure in the decentralisation of cities. We expect that roads and highways are complementary to car ownership, and both are required to facilitate decentralisation. Although we do not have an instrument for road length, in Table 5.B.5 we examine whether including the log of road length as a control variable meaningfully impacts our coefficient of interest.[187] If car manufacturing countries built significantly more roads and highways, it is likely that our IV results will decline as part of the effect may be captured by the

[187] Road length is measured as the total centre-line kilometres or miles of all public roads, or segregated express roads in the case of highways, therefore we may have measurement error as multiple lane roads are counted the same as single lane roads. Furthermore, several missing observations for

Table 5.B.5: 2SLS sensitivity checks: Roads

	Dep var: Population density (log)					
	(1) Subset roads	(2) Road leng.	(3) Subset HW	(4) HW leng.	(5) Subset	(6) Roads&HWs
Cars per 100	-0.0311***	-0.0258***	-0.0277***	-0.0256***	-0.0299***	-0.0253**
	(0.0102)	(0.00953)	(0.00954)	(0.00976)	(0.00975)	(0.00998)
Road length (log)		-0.140***				-0.135**
		(0.0364)				(0.0525)
Highway length (log)				-0.0814*		0.0126
				(0.0453)		(0.0660)
Controls	Y	Y	Y	Y	Y	Y
Decade FE	Y	Y	Y	Y	Y	Y
First-stage F-statistic	16.03	15.52	18.73	17.90	18.91	17.35
No. of countries	50	50	44	44	44	44
No. of cities	108	108	94	94	94	94
No. of observations	200	200	124	124	120	120

Notes: Estimates are weighted by the number of observations per city. Controls are the log of GDP per capita, 1920 population density, January and July temperature, annual precipitation, altitude, ruggedness, and legal origin fixed effects, as in column (5) of Table 5.2. Robust standard errors are in parenthesis and are clustered at the country level. Kleibergen-Paap F-statistic is presented. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

roads coefficient. Column (2) indicates that including the log of road length reduces the size of the 2SLS coefficient by around 20% while the F-statistic is around the same size and the coefficient on road length is negative and statistically significant at the 1% level. Columns (4) and (6) also suggest that highways reduce the effect of cars on population density in our 2SLS estimates, however highway length appears to be less important than overall road length.

5.B.5 Sensitivity to various data sources

Our data is composed of three main data sources, obtained from Ingram and Liu (1999), Kenworthy and Laube (2001), and UITP (2015). In Table 5.B.6, we assess the sensitivity of our results to the various data sources. In columns $(1)-(3)$, we separately estimate the effect of car ownership rates on population density, including all main controls, on each dataset. There are two important observations from this exercise. Firstly, the first-stage F-statistic appears to decline and secondly, the long-run effect of car ownership rates appears to decline. This may be due to changes over time, or due to the inclusion of additional cities and countries in the analysis.

road length and highway length is not present in the Ingram and Liu (1999) data, therefore the relevant comparison group is the subset roads and subset HW columns.

Table 5.B.6: 2SLS Sensitivity checks: Data sources

	Dep var: Population density (log)				
	(1)	(2)	(3)	(4)	(5)
	Ing & Liu	MCD 1995	MCD 2012	Ing & Liu cities	No weights
Cars per 100	-0.0744***	-0.0347***	-0.0117	-0.133***	-0.0357***
	(0.0141)	(0.0121)	(0.0160)	(0.0500)	(0.0117)
Controls	Y	Y	Y	Y	Y
Decade FE	Y	Y	Y	Y	Y
First-stage F-statistic	25.22	14.62	6.531	5.003	20.03
No. of countries	18	51	39	18	57
No. of cities	35	100	63	31	123
No. of observations	69	100	63	50	232

Notes: Estimates are weighted by the number of observations per city. Controls are the log of GDP per capita, 1920 population density, January and July temperature, annual precipitation, altitude, ruggedness, and legal origin fixed effects, as in column (5) of Table 5.2. Robust standard errors are in parenthesis and are clustered at the country level. Kleibergen-Paap F-statistic is presented. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Therefore in column (4) we estimate the effect on the same cities as in Ingram and Liu (1999), but for later periods. The first-stage F-statistic goes down, while the magnitude increases, suggesting that car manufacturers were more powerful in the 1960s and 1970s in influencing policy, which is in line with anecdotal evidence. The reduction in the magnitude of the second-stage coefficient appears to be driven by the inclusion of other cities and countries. This suggests that the sample of cities in Ingram and Liu (1999) is not completely representative.

5.B.6 Other robustness checks

We test the sensitivity of the results to various alternative specifications and report the results in Table 5.B.7. Car manufacturers in 1920 tended to be present in higher income countries. If commercial car manufacturers were more likely to have begun in countries that had larger, more developed, markets in 1920, the instrument may be correlated to the rate of urbanisation and thereby population density in later years. Therefore, in column (1) we include the log of GDP per capita and population size *at the country level* in 1913 as additional controls. The first-stage F-statistic falls to 10.25 and the estimate becomes imprecise, but the magnitude of the point estimate increases slightly and is close to our preferred specification, suggesting that our main result still holds.

Table 5.B.7: 2SLS sensitivity checks: Alternative specifications

	Dep var: Population density (log)				
	(1) Hist. controls	(2) Ex. US	(3) Ex. East bloc	(4) IV1910	(5) IV1930
Cars per 100	-0.0334	-0.0200*	-0.0247**	-0.0196*	-0.0333***
	(0.0206)	(0.0117)	(0.0100)	(0.0116)	(0.0110)
GDP per cap 1913 (log)	-0.0438				
	(0.237)				
Pop 1913 (log)	0.0661**				
	(0.0286)				
Controls	Y	Y	Y	Y	Y
Decade FE	Y	Y	Y	Y	Y
First-stage F-statistic	10.25	12.36	22.23	10.46	25.13
No. of countries	57	56	54	57	57
No. of cities	123	111	119	123	123
No. of observations	232	205	226	232	232

Notes: Estimates are weighted by the number of observations per city. GDP per capita and population in 1913 are at the country level. Controls are the log of GDP per capita, 1920 population density, January and July temperature, annual precipitation, altitude, ruggedness, and legal origin fixed effects, as in column (5) of Table 5.2. Robust standard errors are in parenthesis and are clustered at the country level. Kleibergen-Paap F-statistic is presented. Statistical significance is denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Just over 10% of the observations in our dataset (12 cities and 27 observations) come from the US, which may be an outlier due to their firmly established car culture and the abundance of land for urban expansion, so in column (2) we exclude US cities. The findings suggest that the effect size decreases only slightly to -2% and becomes less precise (statistically significant at the 10% level).

Country borders changed throughout the twentieth century. This may affect the strength of the instrument if some countries where treated with a car manufacturer in 1920 or in a subsequent period, but became independent thereafter. In our dataset, this only affected countries formerly part of the USSR and potentially countries in the Eastern block, and includes Estonia, Poland and Hungary. Therefore in column (3) we exclude these countries, however the effect size is essentially identical and the first-stage F-statistic increases to 22.

In columns (4) and (5) we test the robustness to specifying the IV at different time periods (in 1910 and 1930, respectively). In 1910, only Australia did not have a car manufacturer while in 1930, both Sweden and Japan also had car manufacturers. In both cases the point estimate does not significantly change. Excluding Australia appears to reduce the strength of the instrument to *F*-statistic $= 10$ and the point estimate becomes -2% (statistically significant at the 10% level), while including Sweden and Japan increases the first-stage *F*-statistic to 25 and the point estimate becomes -3.3%.

6
Conclusion

This dissertation presents four empirical analyses on the urban economic effects of private vehicles. These analyses contribute to the current debate on urban and transport policy covering topics ranging from the efficacy of hourly and residential parking prices to the implications of in-vehicle distractions on road safety and the long-term impact of cars on urban density. In each case, the empirical estimates also contribute to a better understanding of how automated vehicles (AVs) may impact our cities in the future. Section 6.1 summarises the main conclusions, Section 6.2 discusses the main implications for urban and transport policy, and Section 6.3 outlines the implications for AVs.

6.1 Main results

Chapter 2 examines the effect of parking prices on hourly parking demand and traffic in the city of Amsterdam. Using variation from a sudden citywide increase in hourly on-street parking prices in 2019 of 66%, we estimate citywide price elasticities of demand for hourly on-street and off-street parking using an event study approach. Our findings show that overall on-street parking demand fell by around 17%, while the combined demand for the entire hourly parking market (on-street and off-street) declined by 14%. We do not find that the reduction in on-street parking is offset by an increase in demand off-street. Our results indicate that on-street parking arrivals declined by 9%. This implies a decrease in overall traffic flow of around 2% – 3%, which is confirmed with traffic flow data. We also estimate the impacts on traffic within the day and find larger traffic effects during the afternoon peak. This is caused by the market structure for on-street parking as the combined number of arrivals and exits is largest between 12:00 – 18:00, and because drivers are more responsive to parking prices in the evening, which results in larger on-street parking elasticities.

Chapter 3 studies the effects of residential parking prices on car ownership in the metropolitan areas of Amsterdam, Rotterdam, Utrecht, and The Hague. We propose a new methodology to estimate implicit residential parking prices using a hedonic house price approach and geographically weighted regressions. We then examine the effect of these parking prices on household car ownership using a MNL control function approach. Our results indicate that for city centres, annual residential parking costs are around €1000, or roughly 17 percent of car ownership costs. Households facing a one standard deviation (€503) increase in annual parking costs own 0.085 fewer cars on average, corresponding to a price elasticity of car demand of about -0.7.

Chapter 4 examines the effect of smartphone distractions on road accidents in the Netherlands. Identification is based on exogenous variation from the *Roam Like at Home* (RLAH) policy which resulted in substantially lower phone usage fees for roaming users (people travelling abroad within the EU) as all roaming surcharges were eliminated. This allows for the estimation of a difference-in-differences model where non-Dutch drivers from the EU are treated, while Dutch drivers serve as the control group. We first document a substantial increase in overall roaming usage, especially data use (MBs per subscriber) which increased by 200 percentage points relative to locals. The results suggest that the increase in mobile phone use by roaming users, due to the policy, caused the number of vehicles involved in accidents to increase by around 10% and that these accidents mainly happen on urban roads. This implies a crash risk odds ratio associated with mobile phone use of around 3.8, which is larger than earlier field studies performed on data before the prevalence of widespread smartphone adoption.

Finally, Chapter 5 investigates the long-run impact of car ownership on urban population density, based on a sample of 123 cities in 57 countries. Using the presence of a car manufacturer in 1920 as a source of exogenous variation, our IV estimates indicate that car ownership substantially reduces urban density. A one standard deviation increase in car ownership rates causes a reduction in population density of around 40%. The results are similar for employment density and we find that the effect appears to be driven by expansions in the built-up area, rather than population leaving the city, suggesting that cars facilitate substantially lower density urban development in the periphery.

6.2 Implications for urban and transport policy

The four main sections of this thesis examine how private vehicles impact urban mobility. This section outlines the key take-aways for policy makers and discusses some of the practical considerations that transport modellers should take into account when applying these estimates. Section 6.2.1 and 6.2.2 discuss the implications for hourly and residential parking policy. Section 6.2.3 discusses the implications for road safety and Section 6.2.4 discusses the long-term implications of car use on urban density.

6.2.1 On-street hourly parking policy

Chapter 2 shows that hourly on-street parking prices are an effective tool to reduce overall traffic in the city while generating a substantial increase in government revenues. This supports the theoretical literature which indicates that parking prices are an integral part of urban transport policy. However, as theory suggests and our empirical estimates show, parking policy is not a silver bullet. In Amsterdam, only about one quarter of traffic in the city uses paid (hourly) on-street parking, with the rest of traffic composed of motorists parking in off-street garages, residents (that receive cheap permits from the municipality), commuters (that often receive free parking from their employers), shared transport (taxi's, buses, and shared cars), and delivery vehicles. Our estimates indicate that increasing on-street parking prices by 50% causes the number of parking arrivals and exits to decline by around 8% and traffic flows by around 2%. Although one positive consequence of the policy is that the effects on traffic flow are larger during the evening peak hour between 16:00 – 20:00 than at other times, the overall impact on traffic and congestion is constrained by the share of traffic that uses hourly on-street parking.

When applying this estimate to other cities, policy makers should consider (1) the share of traffic that will be impacted by the policy, (2) the responsiveness of drivers to changes in parking prices, and (3) the absolute level of the current pricing regime. First, the percentage of traffic that parks on-street determines the size of the group that will be affected by the policy (in Amsterdam around one quarter). Second, the price elasticity of parking depends on the availability of substitutes (parking as well as other transport options). Amsterdam has very good quality substitutes to the car, including world-class cycling infrastructure and an excellent public transport network, but also has a competitive off-street parking market. If substitutes to the car are of lower quality or if motorists are less responsive to price changes, such as motorists with a work purpose, the effects are likely to be smaller. Third, it is important to highlight that on-street parking prices in Amsterdam were already high (compared to world standards) and are similar to off-street prices. In cities where on-street parking prices are close to zero or significantly different to off-street prices, the effects may be larger or smaller due to the corresponding reduction in cruising for parking and potential non-linearities in price effects. This would be a useful direction for further research.

6.2.2 On-street residential parking policy

Local governments can also target traffic generated by other users, for example residents. In Chapter 3 we provide an estimate of the impact of residential parking costs on car ownership. Policy makers can directly impact parking costs via on-street residential parking permits. Other studies have shown that these residential parking permits are often offered to residents for prices far below the market rate, which is likely to result in excess car ownership. We estimate that excess car ownership in the city centre of Amsterdam may be as large as 24%, which is associated with annual welfare losses per household of around €300. Closing this gap would result in additional reductions to traffic as residents account for around half of within city car trips.

In order to apply this estimate to other cities, transport modellers should take into account cruising costs, vehicle costs, and the urban context. First, the parking costs we estimate are composed of residential permit prices and private cruising costs. As residential permit prices increase, this will result in less cruising as fewer households own a car. Therefore, raising residential permit prices is likely to be less effective than overall residential parking costs at reducing car ownership and depend both on the size of cruising costs and the effect of raising prices on cruising costs. Further research should attempt to exploit quasi-experimental variation in residential parking prices over time in order to examine these dynamic changes. Second, we calculate the elasticity with respect to the overall costs of car ownership, which is likely to differ between countries. When calculating the effects of raising residential parking costs, modellers should take overall annual costs into account, which in the case of the Netherlands, approximately equals €5,000 (excluding residential parking). Third, urban context is important. Residential parking prices in the cities we study in the Netherlands are already higher than many other cities around the world, few households have access to private garages, and there are good quality substitutes. The elasticity is likely to be lower in cities where residential parking prices are cheaper (or free) and where drivers have fewer substitutes.

Both of these parking studies also provide policy makers with tools to free up valuable space in cities currently designated to on-street parking. Many cities around the world are currently experimenting with transforming on-street parking into various other uses in order to improve the quality of urban space. This includes adding more greenery (Lisbon), improving cycling infrastructure (Amsterdam), and expanding the outside areas of cafes, bars, and restaurants (Paris). It is important to implement demand-side policies (tackling prices) alongside supply-side policies (removing parking) to avoid the detrimental consequences from additional cruising for parking, which results in more congestion, pollution, and accidents.

6.2.3 Road safety policy

Chapter 4 provides evidence for the size of the detrimental effects of driver distractions on accidents in the Netherlands. Experimental studies suggest that drivers in a foreign environment are not more behaviourally impacted by phone use. Given that our estimate captures the effect of an increase in phone use *due to the RLAH policy*, which was not entirely absent before roaming surcharges were eliminated, this implies that 10% may be an underestimate of the total effect of phone use on accidents.

Our estimates for the Netherlands then imply that phone distractions may be responsible for at least 13,000 additional accidents annually, of which about 2,500 result in injury, and 79 are fatal. Assuming that the effects carry over to other drivers in Europe, the results suggest that around one-third of the gap between realised safety improvements and the EU target in 2018 could have been closed by successfully banning mobile phone use while driving. We calculate that the range of plausible relative crash risks implied by our estimate is between 2 – 6. This is slightly lower in magnitude than those found for positive levels of blood alcohol (relative risk of 7 – 13) and suggests that you are two to six times more likely to be in an accident when using a mobile phone. These findings corroborate earlier literature which suggests that mobile phone bans alone are not enough to eliminate the detrimental effects on road safety. Policies targeting enforcement or social compliance, such as monitoring phone use and an effective public health campaign, may result in substantial safety improvements on roads.

Several aspects of the Dutch context need to be discussed. Roads in the Netherlands are relatively safe and infrastructure is designed to minimise accident likelihood, by for example separating cycling paths, replacing intersections with roundabouts, and diverting traffic from narrow urban streets to highways. Our findings suggest that the effects are mainly on urban roads, where there is more variation in the road conditions, while we do not find an effect on highways. This may suggest that better infrastructure could reduce the effects of distractions, however further research is required to determine the impact of phone use in other settings.

6.2.4 Urban spatial planning and vehicle taxation

Chapter 5 finds that additional car ownership results in substantially lower urban density. An increase in car ownership of one car per 100 inhabitants leads to a reduction in population density of around 2.4%. This has implications for low and middle-income countries, where car ownership is low and urban density is high. Applying our estimates to developing Asian countries in our dataset suggests that if car ownership increases to similar rates as seen in Western Europe, urban density will fall by around 50% in the long-run, with larger reductions if car ownership rates reach levels seen in North America and Oceania. Furthermore, we apply the estimates to three Chinese cities and find that between 1995 and 2010, up to 50% of the reduction in population density can be attributed to changes in access to cars.

Several aspects should be considered when applying this estimate to a particular city. First, the stringency of land use regulations on new developments and the availability of developable land may affect the ability for the urban area to expand outward and thereby impact the size of the estimated effect. Furthermore, other transport policies such as parking pricing, road pricing, and investing in good quality transport infrastructure are likely to reduce the dependence on automobiles and thereby decrease the effect of car use on urban density.

6.3 Implications for AVs

We now turn to a discussion of the implications for AVs in the absence of policy intervention. As discussed in Section 1, AVs are likely to impact mobility and thereby urban economic outcomes via changes in *parking, safety, cost, and convenience*. The studies in this dissertation provide order of magnitude estimates for the potential spatial economic impacts of AVs via the mechanism of *lower hourly and residential parking prices, eliminating phone distractions, and increased access to cars.*

One key effect of AVs is via parking demand. AVs are expected to be able to park autonomously and without a driver in the vehicle, thereby decoupling parking demand with a passengers destination. As parking prices vary strongly within space, traffic demand is expected to increase more in dense urban centres, because parking prices in these areas are currently higher. Chapter 2 provides an indication of the extent to which road traffic is expected to increase *via lower hourly parking prices*. The estimates indicate that if prices were to fall dramatically because AVs no longer require parking at a passengers final destination, traffic demand in Amsterdam is expected to increase by 15% – 33% in the city centre, depending on whether AVs are shared or privately owned.

Chapter 3 provides an indication for the impact of AVs on residential car demand *via lower residential parking costs* as households will no longer require parking directly outside their residence. Our estimates provide long-run approximations for the effect of AVs on cruising costs and vehicle demand. The findings indicate that the average annual welfare gain per household from not incurring residential parking costs is between €450 and €850 in the city centre, depending on whether AVs are privately owned or shared. This is associated with an increase in car demand in the city centre of 8 to 14 percent, respectively. These effects are predominantly driven by gains from eliminating private cruising costs and are smaller outside central urban areas where parking costs are lower. Given that annual average travel distances per car are approximately 13,000 km, additional vehicle demand may result in up to 1,600 km of additional annual car use by households in city centres.

Chapter 4 provides an indication for the potential safety benefits from AVs *via fewer phone induced vehicle accidents*. Computers are expected to be more reliable than the average human driver and therefore AVs are likely to improve road safety. Our findings indicate that 10% of all vehicle accidents in the Netherlands are caused by phone distractions. This implies that AVs could result in about 13,000 fewer vehicle accidents annually, of which about 2,500 result in injury and 79 are fatal. Most of these accidents occur in urban areas, which suggests substantial welfare gains for urban residents.

Chapter 5 provides an indication of the extent to which AVs will result in decentralisation of cities *via increased access to cars*. Currently, car use is constrained by ownership. However, AVs are expected to reduce the fixed and variable costs of using a car, thereby substantially increasing vehicle access. Taking assumptions on changes in vehicle kilometres travelled (VKT) from the literature, our estimates suggest that if VKT increases by 10% – 20%, population density is expected to decline by 6.5% – 12.5% in the long-run.

These findings point towards several directions for urban and transport policy. Having lost an important source of revenues from parking, governments may opt for a more comprehensive solution to mitigating the congestion, pollution, and accident externalities from vehicle travel, while filling a public financing gap. This could be achieved through road pricing, which is the preferred economic approach to tackle traffic externalities. AVs will be easier to track than traditional vehicles which may remove technical challenges facing the adoption of road pricing. Furthermore, shared taxi companies such as Uber and Grab already charge higher fares during peak hours, so road pricing may become more socially acceptable. Our results suggest that a shared AV scenario is more socially desirable, however by no means is this transition automatic. If vehicle externalities continue to go unpriced, AVs may exacerbate the traffic issues we currently face today. These studies show that effective public policy remains crucial to maximise the net benefits of an AV future.

Summary

This dissertation presents four empirical analyses on the urban economic effects of private vehicles. These analyses contribute to the current debate on urban and transport policy, covering topics ranging from the efficacy of hourly and residential parking prices, to the implications of in-vehicle distractions on road safety, and the long-term impact of cars on urban density. In each case, the empirical estimates are applied to improve our understanding of how automated vehicles (AVs) may impact our cities in the future.

In cities, parking occupies a large share of land and is often provided to residents and visitors at prices below the market rate. According to economic theory, this causes excess car ownership and use; however, we lack well-defined quantitative estimates of these effects. Chapter 2 examines the effect of a large citywide increase in hourly on-street parking prices on parking and traffic demand in Amsterdam. Our findings indicate that the citywide increase in hourly on-street parking prices in 2019 of 66%, resulted in 9% fewer on-street parking arrivals and an overall reduction in traffic flow of around 2% – 3%.

Chapter 3 focuses specifically on residents and examines how residential parking prices affect car ownership decisions. Our results indicate that for city centres, annual residential parking costs are around €1000, or roughly 17 percent of car ownership costs, and are more than double the costs in the periphery. Households facing one standard deviation (€503) higher annual parking costs own 0.085 fewer cars, corresponding to a price elasticity of car demand of about -0.7. This implies that the disparity in parking costs explains around 30% of the difference in average car ownership rates between the city centre and the periphery. These two chapters support the abundant theoretical literature, which indicates that parking prices are an integral part of urban transport policy.

Chapter 4 studies how the rise in smartphone use over the past decade has impacted road safety in the Netherlands. Our results suggest that about 10% of vehicle accidents are caused by smartphone use and that these accidents mainly happen on urban roads. The findings imply that you are about 3.8 times more likely to cause an accident if using a mobile phone, which is larger than earlier field studies performed on data before the prevalence of widespread smartphone adoption.

Chapter 5 studies the long-term effect of cars on urban density. Using a global sample of cities, we find that a one standard deviation increase in car ownership rates causes

a reduction in population density of around 40%. This effect appears to be driven by expansions in the built-up area, suggesting that cars facilitate lower density development in the periphery.

AVs are expected to result in improvements to accessibility and safety, increases in car demand, and a redistribution of people and jobs over space. Chapters 2 and 3 demonstrate how lower parking prices are expected to result in more traffic and vehicle demand within cities. Chapter 4 provides an indication for the potential safety benefits from AVs due to fewer smartphone distractions. Finally, Chapter 5 examines how increases in vehicle access and demand, due to cheaper and more comfortable car travel, are expected to impact urban population density in the long-run.

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The Tinbergen Institute is the Institute for Economic Research, which was founded in 1987 by the Faculties of Economics and Econometrics of the Erasmus University Rotterdam, University of Amsterdam and VU University Amsterdam. The Institute is named after the late Professor Jan Tinbergen, Dutch Nobel Prize laureate in economics in 1969. The Tinbergen Institute is located in Amsterdam and Rotterdam. The following books recently appeared in the Tinbergen Institute Research Series:

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